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The Origins of the French Nobility: A Reassessment

CONSTANCE B. BOUCHARD

SINCE THE TWELFTH CENTURY there has been interest in the question of the origins of the French nobility of the High Middle Ages: were these nobles “new” men—recent descendants of soldiers of fortune—or were they the direct descendants of the dukes and counts of Carolingian or even Merovingian times? Scholarly consensus long inclined toward the former opinion, but in the last twenty-five years a number of historians have sought to establish the latter. In all cases, the question has been treated as a strict “either-or” issue: if nobles were “new,” they could not in any way be descended from the Carolingian nobility, or, conversely, if nobles’ pedigrees had any discernible origins in the nobility of the ninth century or earlier, then they could not be considered “new” nobles. This essay will re-examine this question and attempt to resolve this apparent dichotomy by tracing a number of noble lineages, especially the marriage connections, which provide a valuable but so far unutilized index to noble origins.

Anyone following the chronicles and charters of one region from the ninth century through the eleventh or twelfth century must be struck by the increase in the number of nobles found in the sources, of families whose members are designated as *nobilis*, *dominus*, *vir illustre* or *praeclarus*, or by the titles *comes* or *dux*.¹ In the ninth century, the nobility formed a very small group;² by the twelfth century, every region included dozens of noble lineages. Throughout this period, the nobility was considered an elite; nobles were set above the rest of the society

An abbreviated version of this essay was presented as a paper at the Fifty-Fifth Annual Meeting of the Medieval Academy of America, held in Los Angeles, March 1980.

¹ For the concept of *nobilitas*, especially see Jane Martindale, “The French Aristocracy in the Early Middle Ages: A Reappraisal,” *Past & Present*, no. 75 (1977): 5–45, and the works cited therein (although her study has to a large extent superseded earlier works). Important discussions of the topic are also found in Wilhelm Störmer, *Früher Adel: Studien zur Politischen Führungsschicht im Fränkisch-Deutschen Reich vom 8. bis 11. Jahrhundert*, Monographien zur Geschichte des Mittelalters, vol. 6 (Stuttgart, 1973), 13–28; E. Warlop, *The Flemish Nobility before 1300*, trans. J. B. Ross, 1 (Kortrijk, 1975): 40; Léopold Gênicot, “La Noblesse au moyen âge dans l’ancienne ‘Francie,’” *Annales: Économies, sociétés, civilisations*, 17 (1962): 1–8; and P. Bonenfant and G. Despy, “La Noblesse en Brabant aux XII^e et XIII^e siècles,” *Le Moyen Âge*, 64 (1958): 27–66.

² René Poupardin, “Les Grandes Familles comtales à l’époque carolingienne,” *Revue historique*, 72 (1900): 72–95; Otto Forst de Battaglia, “The Nobility in the European Middle Ages,” *Comparative Studies in Society and History*, 5 (1962–63): 64; Michel Parisse, “La Noblesse Lorraine, XI^e–XIII^e siècles” (Doctoral thesis, Université de Nancy II, 1975), 305; and Wilhelm Störmer, *Adelsgruppen im früh- und hochmittelalterlichen Bayern*, Studien zur Bayerischen Verfassungs- und Sozialgeschichte, vol. 4 (Munich, 1972), 13, 182–87.

because of their wealth, power, and exalted birth. The recent work of Jane Martindale and others indicates the stability of the concept of nobility throughout the early Middle Ages (nobility was not strictly equivalent to knighthood, vassalage, or liberty, at least through the twelfth century). But membership in this elite constantly increased. In the tenth and eleventh centuries, *nobilitas* was not yet juridically defined, but members of the class and the churchmen who wrote their histories had no trouble distinguishing who was and was not *nobilis*.³ Thus, the question of the origins of the nobility is that of whether this elite constituted an open or closed class—whether, in other words, a family could or could not experience upward mobility and join the nobility.

In the following pages I will seek to demonstrate the upward mobility of several French families into the ranks of the upper nobility. The term “family” is somewhat imprecise because it is not a translation of any medieval term. As used here, it identifies a group of blood relatives conscious of their close ties with each other and their ancestors. A family will be considered part of the nobility if its members are referred to as *nobilis* (or *vir illuster* or *praeclarus*) in the records or, in the absence of such a designation, if they held an important office such as duke or count.⁴

IN THE ELEVENTH AND TWELFTH CENTURIES, it was assumed that the new nobles were truly “new,” men of socially inferior backgrounds who gained their power during the confusion and political vacuum of the invasions of late Carolingian times. The prologue to the twelfth-century *Chronica de gestis consulum Andegavorum* states, “At the time of Charles the Bald, many new and nonnoble men, stronger than the nobles in goodness and virtue, became great and renowned.” The anonymous author went on to describe the first ancestor of the early counts of Anjou as being a forester and country person (*habitor rusticanus*) and the father of a dependent of Charles the Bald (*cliens, casatus*), who was himself the father of a serving knight (*miles*) of Louis II.⁵ The author’s use of terms—*cliens* and *miles*,

³ The thorny problems of whether the nobility constituted a juridic class and whether nobility was identical to knighthood, vassalage, or liberty have been debated at length by scholars from P. Guilhaume at the beginning of this century to Marc Bloch, Bonenfant and Despy, Gerd Tellenbach, Georges Duby, and, most recently, Martindale. Bonenfant and Despy, “La Noblesse en Brabant,” 27–29, 36–37, 40; Duby, *La Société aux XI^e et XII^e siècles dans la région mâconnaise* [hereafter, *La Société mâconnaise*] (2d ed., Paris, 1971), 192–94; and Tellenbach, “Zur Erforschung des hochmittelalterlichen Adels (9.–12. Jahrhundert),” in Comité internationale des sciences historiques, *XII^e Congrès internationale des sciences historiques, Rapports*, vol. 1: *Grands Thèmes* (Vienna, 1965), 320–24. For Guilhaume and Bloch, see note 8, page 503, below. On the distinction between knights and nobility, see note 7, page 503, below; and Sally Harvey, “The Knight and the Knight’s Fee in England,” *Past & Present*, no. 49 (1970): 3–43. Being noble was, of course, a somewhat different proposition in the later Middle Ages, when the ceremony of knighting was a sign of nobility and when the king could ennoble those who served him. The present essay is concerned only with the earlier period; for a survey of the thirteenth through fifteenth centuries, see Robert H. Lucas, “Ennoblement in Late Medieval France,” *Medieval Studies*, 39 (1977): 239–60.

⁴ Constance B. Bouchard, “The Structure of a Twelfth-Century French Family: The Lords of Seignelay,” *Viator*, 10 (1979): 41–44. George Beech has found, working with materials from Poitou, that before the twelfth century a count was rarely designated as *nobilis*, his nobility being self-evident from his comital title; Beech, “Prosopography,” in James M. Powell, ed., *Medieval Studies: An Introduction* (Syracuse, 1976), 168.

⁵ “Igitur tempore Karoli Calvi, complures novi atque ignobiles, bono et honesto nobilibus potiores, clari et

for example, which in the twelfth century indicated someone in a socially inferior position to the *domini* and *nobiles*⁶—shows that, whether or not he was accurate about the counts' origins, he considered them examples of the "new" men who became "great and outstanding." A century earlier, Aimon of Fleury had expressed a similar view of the origins of the new nobility when he said that Robert the Strong, the ancestor of the Capetian kings, had attained his power when the king left him, a dependent soldier, to fight the Norsemen and the Magyars, while retreating himself. The historian Richer, Aimon's contemporary, referred to Robert as a simple knight, "ex equestri ordine."⁷ This view of the new nobility as stemming from knights and nonnobles who appropriated power during the ninth and tenth centuries was accepted from the eleventh and twelfth centuries down to the twentieth, when Marc Bloch gave it its most forceful statement.⁸

In the last twenty-five years, many scholars have challenged Bloch's view and attempted to show that members of the "new" French nobility of the eleventh and twelfth centuries were, in fact, the direct descendants of the "old" nobility, the ninth-century counts and officials of Charlemagne's court, and that their position came not from an appropriation of power but from their illustrious ancestry. For example, E. Warlop and Léopold Gênicot, both working with sources from the Low Countries, have concluded that the nobility was established there as a class of powerful landowners from the early Middle Ages and underwent little change in composition until the thirteenth century; both, however, recognize that this point is hard to demonstrate directly, as almost no noble genealogies can be traced earlier than the year 1000. Karl Ferdinand

magni effecti sunt"; Louis Halphen and René Poupardin, eds., *Chroniques des comtes d'Anjou et des seigneurs d'Amboise* (Paris, 1913), 25–29. The author seems to have borrowed the phrase from Sallust, though changing its meaning: Sallust was speaking of new men who cared little for goodness and virtue, the twelfth-century author of new men made strong by goodness and virtue; Sallust *Jugurtha* 8. 1.

⁶ An early twelfth-century author, distinguishing among the upper, middle, and lower orders of society, put a count in the upper order but a *miles* in the middle category, and a *cliens* and a *rusticus* in the lowest one; Giles Constable, "The Structure of Medieval Society according to the *Dictatores* of the Twelfth Century," in Kenneth Pennington and Robert Somerville, eds., *Law, Church and Society: Essays in Honor of Stephan Kuttner* (Philadelphia, 1977), 255, n. 11.

⁷ Aimon of Fleury, *Miracula Sancti Benedicti*, 2. 1, ed. E. de Certain, *Les Miracles de Saint Benoît* (Paris, 1858), 92–93; and Richer, *Historia*, 1. 5, ed. Robert Latouche, 2 vols. (Paris, 1930–37), 1: 16. The armed attendants designated as *miles* in the eleventh and early twelfth centuries almost never carried any of the noble titles I have given above. Most modern scholars agree that, although nobles were often knighted, simple knights were sharply distinguished from the nobility until at least 1150. For surveys of the arguments, see Warlop, *The Flemish Nobility before 1300*, 11–17; Lucien Musset, "L'Aristocratie normande au XI^e siècle," in Philippe Contamine, ed., *La Noblesse au moyen âge, XI^e–XV^e siècles: Essais à la mémoire de Robert Boutruche* (Paris, 1976), 88–94; Parisse, "La Noblesse Lorraine," 250–69; and Theodore Evergates, *Feudal Society in the Bailiarge of Troyes under the Counts of Champagne* (Baltimore, 1975), 144–50. Georges Duby, the leading proponent of the view that knights were part of the nobility, has still found a clear distinction until the final decades of the twelfth century between the elite of *domini* and the socially inferior *militēs*; Duby, "Situation de la noblesse en France au début du XIII^e siècle," *Tijdschrift voor Geschiedenis*, 82 (1969): 309 ("The Transformation of the Aristocracy: France at the Beginning of the Thirteenth Century," in his *The Chivalrous Society*, trans. Cynthia Postan [London, 1977], 178).

⁸ Bloch, *Feudal Society*, trans. L. A. Manyon, 2 (Chicago, 1961): 283–92. Bloch was influenced by, although he went far beyond, the concepts of P. Guilhaumez, who believed that nobles were those who served the function of warriors; Guilhaumez, *Essai sur l'origine de la noblesse en France au moyen âge* (Paris, 1902), 1–4, 350–69. Also see Martindale, "The French Aristocracy in the Early Middle Ages," 8, n. 12; and Giovanni Tabacco, "Su nobilità e cavalleria nel medioevo: Un Ritorno a Marc Bloch?" *Rivista storica italiana*, 91 (1979): 9–10.

Werner has offered the strongest challenge to the idea of a "new" nobility in studies designed to show the social and biological continuity of the nobility from the time of the first Carolingian kings to the twelfth century. He has attempted to demonstrate continuity of lineages where there is no standard genealogical evidence by using the evidence of names, *Leitnamen*, presumed to be repeated from generation to generation. Werner has dismissed the twelfth-century belief in a "new" nobility as mere "wishful thinking" (*Wunschtraum*) with no basis in fact. Following Werner, Georges Duby and Jacques Boussard have also set out to demonstrate that many of the French noble families of the eleventh and twelfth centuries had at least one ancestor among the Carolingian nobility.⁹

Although these views have recently gained broad acceptance,¹⁰ they have not yet become "received opinion," chiefly because of the work of Karl Schmid and Gerd Tellenbach. Schmid has argued convincingly that medieval men only became conscious of membership in a continuing family unit when a direct line of inheritance began in offices and castles; at that point their consciousness became identified with the father-son-grandson line that inherited and held the office in turn. This concept of family structure, new in the tenth or eleventh century, has been confirmed by the research of other scholars; indeed, Tellenbach has used it to argue that trying to demonstrate continuities of noble families before the point at which families gained consciousness of themselves as a continuing unit is anachronistic. Rather, the diversity of status of the nobles in the central Middle Ages, he concluded, indicates that there must have been some sort of social mobility—either up or down—during the tenth and eleventh centuries.¹¹

⁹ Warlop, *The Flemish Nobility before 1300*, 43–52; Génicot, *Les Hommes—La Noblesse*, volume 2 of *L'Économie rurale namuroise au bas moyen âge* (Louvain, 1960), 1–22; Werner, "Untersuchungen zur Frühzeit des französischen Fürstentums (9.–10. Jahrhundert)," *Die Welt als Geschichte*, 18 (1958): 256–89, 19 (1959): 146–93, 20 (1960): 87–119, esp. 117–19; Georges Duby, "Lignage, noblesse, et chevalerie au XII^e siècle dans la région mâconnaise," in his *Hommes et structures du moyen âge* (Paris, 1973), 395–442 ("Lineage, Nobility, and Knighthood: The Mâconnais in the Twelfth Century—A Revision," in his *Chivalrous Society*, 59–80), and *La Société mâconnaise*, 317–20; and Boussard, "L'Origine des familles seigneuriales dans la région de la Loire moyenne," *Cahiers de civilisation médiévale*, 5 (1962): 303–22. Werner's insistence on genealogical continuity in the noble class stems from his attempt to prove that the early medieval nobility had an existence separate from the king and was not "re-created" anew by each new royal dynasty; see his "Important Noble Families in the Kingdom of Charlemagne—A Prosopographical Study of the Relationship between King and Nobility in the Early Middle Ages," in Timothy Reuter, ed. and trans., *The Medieval Nobility: Studies on the Ruling Classes of France and Germany from the Sixth to the Twelfth Century*, Europe in the Middle Ages, Selected Studies, vol. 14 (Amsterdam, 1978), 147–49.

¹⁰ Martindale, "The French Aristocracy in the Early Middle Ages," 10–12. John B. Freed, in his work on the origins of the German nobility, felt he had to contrast Germany with France because France, according to the literature cited above, "had no parvenus," but Germany did; Freed, "The Origins of the European Nobility: The Problem of the Ministerials," *Viator*, 7 (1976): 213 n. 7, 241.

¹¹ Schmid, "Zur Problematik von Familie, Sippe und Geschlecht, Haus und Dynastie beim mittelalterlichen Adel," *Zeitschrift für die Geschichte des Oberrheins*, 105 (1957): 4, 15–16, 30–31, "Heirat, Familienfolge, Geschlechterbewusstsein," in *Il Matrimonio nella società altomedievale*, Settimane di studio del Centro italiano di studi sull'alto medioevo, vol. 24 (Spoleto, 1977), 120–30, and "The Structure of the Nobility in the Earlier Middle Ages," in Reuter, *The Medieval Nobility*, 39–42, 48–49; and Tellenbach, "From the Carolingian Imperial Nobility to the German Estate of Imperial Princes," *ibid.*, 204–05, and "Zur Erforschung des hochmittelalterlichen Adels," 321–22, 326–27. Schmid's conclusions have been accepted notably by Georges Duby, K. Leyser, and John B. Freed; see Duby, "La Noblesse dans la France médiévale: Une Enquête à poursuivre," in his *Hommes et structures*, 152–55 ("The Nobility in Medieval France," in his *Chivalrous Society*, 100–03); Freed, "The Formation of the Salzburg Ministerialage in the Tenth and Eleventh Centuries: An Example of Upward Social Mobility in the Early Middle Ages," *Viator*, 9 (1978): 73–74; and Leyser, "The German Aristocracy from the Ninth to the Early Twelfth Century: A Historical and Cultural Sketch," *Past & Present*, no. 41 (1968): 32–33. Leyser has, however, argued that Schmid's reliance on *libri memoriales* gives a picture of early medieval fam-

The theory that the noble class was static may also be questioned from a somewhat different angle: one cannot speak of continuities or discontinuities of a class without discussing its members' marriages. First, however, a brief excursus on *Leitnamen* seems necessary, as most attempts to trace the nobles of the High Middle Ages back to the nobles of Charlemagne's court have been based—erroneously, I believe—on the theory that a noble may be considered a descendant of a noble who lived a century earlier if the two have the same name.

IT IS DEMONSTRABLE that in the eleventh and twelfth centuries children were generally named for their parents, grandparents, and, especially in the case of younger children, aunts and uncles—a practice that provides insight into the concept of family unity prevalent at the time.¹² Maurice Chaume pioneered an approach that uses repetitive naming to reconstruct pedigrees where the standard type of evidence is not available; Karl Ferdinand Werner, though in many respects more cautious than Chaume, has been his most enthusiastic pupil.¹³ In essence, this approach is based on the assumption that no one in the upper levels of society was ever named for anyone but an ancestor and that, therefore, if two men with similar names held property in the same general area at different times, one must be a lineal descendant of the other. Some proponents of this method, including Werner himself, have warned against its overhasty application.¹⁴ The chief difficulties are as follows.

First, it is not at all self-evident that, simply because the same name tended to

ily consciousness more diffuse than that gained from other sources; *ibid.*, 34–36. A picture similar to Schmid's has recently been presented for the Italian nobility; see Cinzio Violante, "Quelques caractéristiques des structures familiales en Lombardie, Emile et Toscane aux XI^e et XII^e siècles," in Georges Duby and Jacques Le Goff, eds., *Famille et parenté dans l'Occident médiévale*, Collection de l'École française de Rome, vol. 30 (Rome, 1977), 93–98. Störmer has arrived at a similar picture of early medieval family structure, although he has downplayed Schmid's suggestion that the adoption of a patronym necessarily accompanied the development of family consciousness; *Früher Adel*, 87–93. On this last point, also see Martin Heinzelmann, "La Noblesse du haut moyen âge (VIII^e–XI^e siècles): Quelques problèmes à propos d'ouvrages récents," *Le Moyen Âge*, 83 (1977): 137–38; and Bouchard, "The Structure of a Twelfth-Century French Family," 44–47. Reinhard Wenskus has suggested that one branch of a family might have developed agnatic consciousness in the eighth or ninth century while their relatives still has a very diffuse family consciousness; Wenskus, *Sächsischer Stammesadel und fränkischer Reichsadel*, Abhandlungen der Akademie der Wissenschaften in Göttingen, philologisch-historische Klasse, ser. 3, vol. 93 (Göttingen, 1976), 470–71.

¹² Bouchard, "The Structure of a Twelfth-Century French Family," 47–49, n. 32; and Karl Schmid, "De regia stirpe Waiblingensium": Remarques sur la conscience de soi des Staufens," in Duby and Le Goff, *Famille et parenté dans l'Occident médiévale*, 51–56. I will not here be discussing the questions of whether naming children for their ancestors was an inherently Germanic pattern or whether these names had a magical-pagan significance; on that question, see Wilhelm Störmer, "Adel und Ministerialität im Spiegel der bayerischen Namentgebung (bis zum 13. Jahrhundert)," *Deutsches Archiv für Erforschung des Mittelalters*, 33 (1977): 88–90; and Karl Ferdinand Werner, "Liens de parenté et noms de personne: Un Problème historique et méthodologique," in Duby and Le Goff, *Famille et parenté dans l'Occident médiévale*, 15–17.

¹³ Chaume, *Origines du duché de Bourgogne*, 1 (Dijon, 1925): 505–53; his discussion has been reprinted in his *Recherches d'histoire chrétienne et médiévale* (Dijon, 1947), 217–27. Bloch called Chaume's lines of filiation "ingenious though improbable conjectures"; *Feudal Society*, 284.

¹⁴ Werner, "Liens de parenté et noms de personne," 14, 25–26, and "Important Noble Families in the Kingdom of Charlemagne," 150–53. Wenskus, in trying to discover "rules" early medieval nobles followed in naming their children, concluded that name similarities can only be used as a key to family ties if two men with the same name (or name-elements) appear together in the documents—for example, when making a joint gift; *Sächsischer Stammesadel*, 41–65.

be repeated from father to son (or grandfather to grandson) in the twelfth century, the same pattern must have applied earlier. Schmid and Tellenbach have argued that families only began to assign their children distinctive hereditary names after the family had assumed a conscious unity and a hereditary castle or county.¹⁵ In the tenth-century records, it is most usual to find such family groups as the couple Romestagnus and Godaltrudis, who had four sons, Alberic, Aymo, Fulcher, and Emmanuel, whose names are not similar.¹⁶ The tenth- and eleventh-century counts of Bologna studied by Cinzio Violante, although they represented a father-son lineage, did not repeat a name for five generations—the first six hereditary counts were Ubaldo, Bonifacio, Adalberto, Walfredo, Ugo, and finally a second Adalberto in the later eleventh century. Even among younger brothers of these counts there is no known repeated name until the fourth generation. Wilhelm Störmer, in his study of the nobility of eighth- and ninth-century Bavaria, found that children there were very rarely named for their parents or grandparents. At most, their names might contain a variation of part of a parental name; for example, one Waltker named his son Kerholt, and Wolfperht was the son of Erchonperht. As Störmer rightly concluded, attempts to construct genealogies on the basis of this evidence alone could only be self-defeating.¹⁷

Second, unrelated lineages may carry the same name: witness, for example, the frequency of the name "William," found in the eleventh century in the lineages of the dukes of Normandy, the dukes of Aquitaine, the counts of Burgundy, and the counts of Provence—since Otto-William, count of Burgundy in the early eleventh century, married his children into the other three lineages, the family connection cannot have been close.¹⁸ It was especially common for two lineages to carry the same name without being related when one lineage represented the dependents of the other lineage. Störmer has found, for example, that, in Bavaria in the tenth through twelfth centuries, those who served in a powerful lord's household, even those who were nonfree, were often given the same names as the lord's own children. Cicely Clark has indicated that the Anglo-Saxon peasantry of England took Continental names in imitation of their

¹⁵ Schmid, "Zur Problematik von Familie," 4; and Tellenbach, "Zur Erforschung des hochmittelalterlichen Adels," 326. Werner has agreed on this point; "Important Noble Families in the Kingdom of Charlemagne," 152.

¹⁶ Though not specifically called noble, this family was certainly rich, giving a great deal of property to the monastery of Savigny, near Forez; Auguste Bernard, ed., *Cartulaire de l'abbaye de Savigny* (Paris, 1853), 113–16, 120–21, 123, nos. 153, 157, 164, 169.

¹⁷ Violante, "Des structures familiales en Lombardie," 147; and Störmer, *Früher Adel*, chap. 2: "Adelige Namengebung in Familie, Sippe und Herrschaft," 29–69. Also see Wenskus, *Sächsischer Stammesadel*, 42–44, 58.

¹⁸ Even early in their ancestry, these families seem to have acquired the name "William" independently of each other. The dukes of Aquitaine, William since the ninth century, acquired the name first chronologically. The name first appears in the lineage of the dukes of Normandy with William Longsword (d. 942), son of the Viking Rollo and a presumably Celtic woman (and thus most likely not a relative of the duke of Aquitaine); see D. C. Douglas, "Rollo of Normandy," *English Historical Review*, 57 (1942): 422–23, 435. Otto-William, count of Burgundy in the early eleventh century, was named for his grandmother Willa. It seems more likely that the counts of Provence took the name William in imitation of—or as godsons of—the dukes of Aquitaine rather than from any blood relationship to them, Berge's proposals to the contrary; J. Berge, *Origines rectifiées de maisons féodales—Comtes de Provence* (Menton, 1952), 40–41.

Norman masters in the generations after the Conquest.¹⁹ The viscounts of Provence took the name "William" in imitation of the counts of Provence; and at the beginning of the eleventh century, when Count William of Provence named his sons Fulk and Geoffrey (after the relatives of his wife, Adelaide of Anjou), Viscount William followed suit about a generation later.²⁰ Even when knights and nonnobles were not named for their direct lord, they were often given the name of a great man of the region. For example, the name Hugh was very frequent in the region around Cluny in the late eleventh century, where St. Hugh was abbot for sixty years; and Werner has found that Heinrich and Konrad, originally names of kings, became the names of every "Tom, Dick, and Harry" ("Hinz und Kunz" in German usage) in the twelfth century.²¹ It should thus be clear that, even assuming continuity of names, the fact that an apparently new lineage carries the same name as a great ninth-century lineage need not indicate direct biological descent; it could equally well indicate that the new nobles were the descendants of the dependent servitors of the nobles of the ninth century.

More minor but still significant problems include difficulties in ascertaining if two Germanic names rendered the same in the Latin of the documents actually are the same or, conversely, if one name rendered in two ways actually means two separate individuals.²² Even when in the twelfth century eldest sons were frequently named for their fathers, it is difficult to know how to attach other members of the family to this purely male line of descent if, due to the identity of names, one cannot be sure when one lord died and his son succeeded. And, of course, boys were often named for the men on their mother's side of the family as well as their father's, especially if the mother's side was the more powerful;²³ therefore, even if a certain nobleman had been named for a glorious ancestor (rather than his lord), there is no guarantee that he was the direct male-line descendant of an old family instead of the son of an upstart who had married a woman of an old family.

An example from one of the best-documented lineages of the eleventh and twelfth centuries demonstrates that, if this lineage had existed in a time when the documentation was sparser, use of names alone would never have served to reconstruct it. Suppose there is a certain king, William I, who was succeeded first by his son William II and then by a younger son, after whose death there is a period of interregnum and civil war. The fighting ends after almost twenty

¹⁹ Störmer, "Adel und Ministerialität," 90-94; and Clark, "Women's Names in Post-Conquest England: Observations and Speculations," *Speculum*, 53 (1978): 236-40.

²⁰ M. Guérard, ed., *Cartulaire de l'abbaye de Saint-Victor de Marseille*, 1 (Paris, 1857): 626-27, 641-42, nos. 630, 649. Also see Noël Coulet, "Autour d'un serment des vicomtes de Marseille," *Annales du Midi*, 91 (1979): 315-19.

²¹ Werner, "Liens de parenté et noms de personne," 26-27. Also see Wenskus, *Sächsischer Stammesadel*, 45-46.

²² On this problem, see Karl Schmid, "Über das Verhältnis von Person und Gemeinschaft in früheren Mittelalter," *Frühmittelalterliche Studien*, 1 (1967): 228-32.

²³ Bouchard, "The Structure of a Twelfth-Century French Family," 48. Erwin Assmann has pointed out that, of the three youngest sons of Frederick Barbarossa, two—Raynold and William—were named after their mother's ancestors, and one—Philip—does not seem to have been named after any ancestor but perhaps one of Frederick's friends; Assmann, "Friedrich Barbarossas Kinder," *Deutsches Archiv für Erforschung des Mittelalters*, 33 (1977): 459-63.

years when a young count ascends the throne, a man who has recently married the daughter of a certain *Wilhelmus dux*. Based on names alone, one would most likely assume that this young king had succeeded due to his wife's hereditary claim to the throne. If, further, this king chose as the companion of his heir another William, who later served as regent for his grandson, one would assume that this William was some collateral relative of the first royal house of Williams. But, in this case, the assumption is false. Henry II of England ascended the throne because he was the grandson of William I's youngest son, Henry I; Duke William X of Aquitaine, his wife's father, entered in no way into his claim to the throne. And William Marshall, the companion of Prince Henry and later regent for Henry III, was not related to the kings of England and, indeed, had origins in a simple knight's family.

In many cases, modern scholars have relied too heavily on name similarities in constructing family trees for ninth- and tenth-century lineages.²⁴ Even careful historians can be trapped in circular reasoning: the assumption that nonnobles would not carry the name of a noble or that nonnobles would never progress to the point where they could be mistaken for nobles of the same name leads to the conclusion that everyone in the same region bearing the name of an old noble family belonged to it and, consequently, that there was no new nobility in that region.²⁵ Other forms of circular reasoning are also possible; one may assume that nobles *never* married their social inferiors and, thus, conclude that anyone with unknown origins who married a noble must also be of noble birth.²⁶ Obviously, this sort of reasoning can lead to a neglect of indications that a new nobility was emerging. Continuity in the common names of noblemen—Gui, Lambert, Boso, or William, for example—does not establish the absence of new nobles.

To be resolved, the question of whether the noble families of the eleventh and twelfth centuries were “new” or “old” must be examined through the histories and family trees of individual families. Many noble families set down their own genealogies in the eleventh and twelfth centuries, and modern scholars have verified and amplified these genealogies by using contemporary charters in which noble donors list their children, wives, and parents. Historians have also constructed family trees for those families that left none. Even when a family tree from the High Middle Ages is well established, there is always a point before which the family line cannot be traced: the modern scholar may lose track of the lineage in the sparse or uninformative records, and the medieval genealo-

²⁴ It should be noted that Werner, in spite of his propensity for *Leitnamen*, has based almost all of the numerous family trees he has constructed on sources independent of name similarities.

²⁵ George T. Beech's work illustrates the difficulties into which this hidden circular reasoning can lead. He took name similarities between people at least a generation apart to show that the viscounts of Thouars had Carolingian origins; but, at most, his work shows that the names the viscounts later took were not unknown in the Carolingian period. The two Aimeris he believed were ancestors of the first Viscount Aimeri appear twenty-eight and eighty years before the viscount, with no indication that they were related or even that the second one was noble. Beech, “The Origins of the Family of the Viscounts of Thouars,” in *Études de civilisation médiévale (IX–XII siècles): Mélanges offerts à Edmond-René Labande* (Poitiers, 1974), 25–31.

²⁶ Berge followed this line of reasoning in his work on the counts of Provence; *Origines rectifiées de maisons féodales*, 28–29.

gists resorted to vague remarks about glorious ancestors in Rome or Troy.²⁷ Almost no French families of the central Middle Ages, with the exception of the Carolingians, can be traced to the eighth century, and only the greatest ducal and comital families (including the Capetians) to the ninth; most other comital families can be traced only to the tenth century, and most castellan families only to the eleventh or early twelfth. The scantiness of genealogical evidence before the tenth century led Störmer to reject all attempts to draw family trees in preference to a study of the groups of nobles who lived at the same time and appear together in the documents.²⁸ But, for all of the difficulties they present, genealogies remain the only way to describe the emergence of noble families. The alternative is to discuss amorphous groups of relatives and presumed relatives, in which all of the difficulties involved in establishing accurate family trees are present, but in exaggerated form.

Figures 1 and 2 show the family trees of several families chosen to demonstrate the origins of the French nobility. All influenced events in Burgundy, the heart of so-called feudal Europe, and all were undoubtedly members of the upper nobility by the twelfth century. By then many had some blood of the Carolingian aristocracy in their veins—whether or not they were aware of it. Werner and other scholars have maintained that the obscurity that often surrounds a noble family's origins is due only to scantiness of documentation—that, if the evidence were better, all of a twelfth-century noble's ancestors could be demonstrated to be of the upper nobility. But marriage patterns in a number of lineages indicate that families arising from stock that was obscure in the full sense of the term were frequently grafted through marriage onto the old nobility and that this happened rather early in their rise to eminence. This marital history explains why historians have frequently found that many families viewed as belonging to the "new" nobility by twelfth-century writers had some ancestors in the Carolingian nobility. Indeed, there are many examples of "unequal" marriages in the tenth and eleventh centuries.²⁹

I will discuss the rise of several noble families chronologically, in the order of their emergence from obscurity in the ninth, tenth, or eleventh centuries. With the benefit of this information, I will return to the question of whether members of these families belonged to the "old" or "new" nobility. Most of the great royal and ducal families of the High Middle Ages first appear in records from the ninth century. I have chosen six of these families as representative of the "old"

²⁷ Léopold Génicot, *Les Généalogies*, Typologie des sources du moyen âge, vol. 15 (Turnhout, 1975), 4; Georges Duby, "Structures de parenté et noblesse dans la France du nord aux XI^e et XII^e siècles," in his *Hommes et structures*, 267–85 ("The Structure of Kinship and Nobility: Northern France in the Eleventh and Twelfth Centuries," in his *Chivalrous Society*, 134–48); Beech, "Prosopography," 158–65; Parisse, "La Noblesse Lorraine," 233–34; and Warlop, *The Flemish Nobility before 1300*, 43.

²⁸ Störmer, *Adelsgruppen im früh- und hochmittelalterlichen Bayern*, 7.

²⁹ James B. Wood has recently posed a similar challenge to the concept of a static noble class in France for the early modern period, arguing that "new" and "old" nobles regularly married each other; Wood, "Endogamy and Mésalliance: The Marriage Patterns of the Nobility of the *Élection* of Bayeux, 1430–1669," *French Historical Studies*, 10 (1978): 375–77. On the arrangement and symbolism of marriage, see Georges Duby, *Medieval Marriage: Two Models from Twelfth-Century France*, trans. Elborg Forster, The Johns Hopkins Symposia in Comparative History, vol. 11 (Baltimore, 1978), and "Le Mariage dans la société du haut moyen âge," in *Il Matrimonio nella società altomedievale*, 13–39.

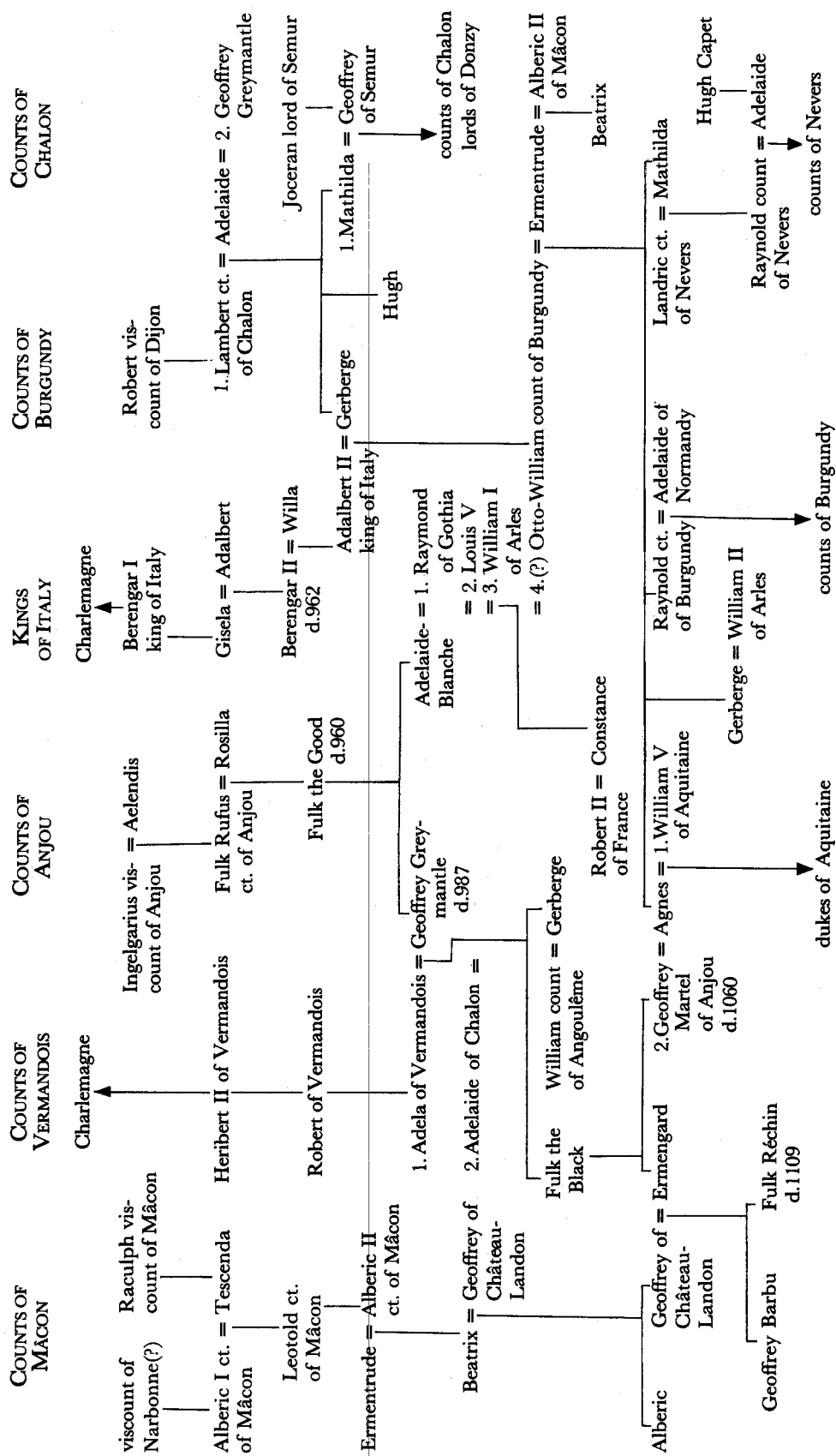


Figure 2. This figure and fig. 1 show the variety of ancestors by the eleventh century among the families mentioned in this essay. There is some overlap within and between the figures.

nobility to which to compare the (relatively) “new” lineages that emerged in the tenth and eleventh centuries. All six of these families influenced events in Burgundy and had intermarried by the tenth century.

ONE OF EUROPE'S OLDEST LINEAGES is that of the Capetian kings of France (which still continues in the Bourbon dynasty of Spain), who traced their origins to Count Robert the Strong (d. 866). He obtained his power while fighting the barbarians for Charles the Bald, and his ancestry is a virtual blank, in spite of many ingenious attempts (few of which have convinced anyone but their creators) to discover his origins. There can be no doubt, however, that his descendants were part of the upper nobility and some of the most powerful men in France.³⁰ His two sons, his granddaughter's husband, his great-grandson, and all succeeding eldest sons (who survived their fathers) were kings of France.³¹ A second great family of the ninth century was that of the dukes of Burgundy, the descendants of Richard le Justicier (d. 921). He, like Robert the Strong, first appears in the sources in the late ninth century resisting the Viking invaders in the name of the Carolingian kings, who had fled to safety. His origins are almost as obscure as Robert's, but his descendants were certainly powerful nobles; his three sons, Raoul, Hugh, and Boson, dominated Burgundy west of the Saône in the first

³⁰ Bloch considered Robert the Strong a prime example of a man rising to power from humble origins, due to his strength and his sword; *Feudal Society*, 284–85. For Robert's wars with the Vikings, see especially Regino of Prüm, *Chronicon*, 861, ed. Friedrich Kurze (1890), *Monumenta Germaniae historica* [hereafter, *MGH*], *Scriptores in usum scholarum*, 79. Also see the *Annales Bertiniani*, 864, 865, ed. Felix Grat et al., *Annales de Saint-Bertin* (Paris, 1964), 112, 123; Richer, *Historia*, 1. 5 (ed. Latouche, 1: 16); and *Annales Floriacenses*, 866, ed. Alexandre Vidier, *L'Historiographie à Saint-Benoît-sur-Loire* (Paris, 1965), 219. A literature refuting attempts to establish Robert's ancestry has grown up parallel to the literature that seeks to prove this ancestry. See, for example, Anatole de Barthélemy, “Les Origines de la maison de France,” *Revue des questions historiques*, 13 (1873): 108–44; Ch. Mauquin, *Histoire des ducs de France* (Paris, 1893), 32–95; J. Calmette, “La Famille de Saint Guilhem et l'ascendance de Robert le Fort,” *Annales du Midi*, 40 (1928): 225–45; L. Levillain, “Essai sur le comte Eudes, fils de Harduin et de Guérinbourg, 845–871,” *Le Moyen Âge*, 47 (1937): 261–71; and J. Dhondt, *Études sur la naissance des principautés territoriales en France (IX^e–X^e siècles)* (Bruges, 1948), 323–24. The only explicit statement in the sources is the remark by Richer, who wrote a century later, that Robert's father was named “Witichinus”; *Historia*, 1. 5 (ed. Latouche, 1: 16). Even if Richer was correct, the question still remains who Witichin might be (and in fact most historians make Robert the descendant of a line of Roberts or Odos); see K. Glöckner, “Lorsch und Lothringen, Robertiner und Capetinger,” *Zeitschrift für die Geschichte des Oberrheins*, 89 (1937): 330–33. Probably the only safe conclusion is that Robert came originally from the Rhineland; see Werner, “Important Families in the Kingdom of Charlemagne,” 172; and Glöckner, “Lorsch und Lothringen,” 333–42. If I may add one argument to the endless discussions of Robert's origins, it appears that the oft-attempted endeavor to link him to the Welfs is fruitless. True, the eleventh-century Chronicle of St.-Bénigne called Hugh the Abbot, himself a Welf, the brother of either Robert the Strong or of his sons: “Supererant duo filii Rotberti Andegavorum comitis, frs Hugonis abbatis”; *Chronica S. Benigni*, ed. E. Bougaud and Joseph Garnier, *Chronique de l'abbaye de Saint-Bénigne de Dijon*, *Analecta Divionensis*, vol. 9 (Dijon, 1875), 109. Scholars have generally expanded the abbreviation to *fratris* and made Robert the brother, brother-in-law, or cousin of Hugh; some have also expanded it to *fratres* and made Robert the step-father of Hugh. In fact, this passage was taken almost verbatim from the earlier *Miracula Sancti Benedicti*, 2. 1 (ed. de Certain, 94). Here, however, there is no mention of Hugh the Abbot; the chronicler of St.-Bénigne seems to have added the phrase, “fratres Hugonis abbatis” (as I prefer to expand the abbreviation) not because he had any intimate knowledge of Robert the Strong's relations but because he assumed that Hugh the Abbot, who was given Robert's property when he died (leaving his sons only boys), must have been Robert's son. Also see *Annales Bertiniani*, 866 (ed. Grat et al., 132); and Levillain, “Essai sur le comte Eudes,” 270, n. 1.

³¹ I have called all of Robert's descendants “Capetians,” although some scholars prefer to call them “Robertians” until the time of Hugh Capet, a century and a half later. For the sake of clarity, I prefer to call this family by only one name; they had no name for themselves.

half of the tenth century, and Raoul married the granddaughter of Robert the Strong and became king of France in 923.³² A third great lineage of the ninth century was that of the counts of Vermandois, an unusual family in that their ancestry in the male line was undoubtedly Carolingian: Charlemagne's son Pippin, king of Italy, had a son Bernard, who had a son Pippin, who was the father of Heribert I, the first of the hereditary counts of Vermandois. These counts married into lineages that were apparently more recently established than their own: Heribert II of Vermandois married a granddaughter of Robert the Strong, sister of the wife of Raoul of Burgundy.³³

The Ottonian kings of Germany and the Rudolphian kings of imperial Burgundy both traced their origins to men who held the office of duke at the beginning of the ninth century. Henry the Fowler, son of Duke Otto of Saxony, was the first of his line to replace the Carolingians on the German throne in 919; his son Otto I made himself western emperor in 962. Henry the Fowler's daughter Gerberge married, first, Duke Giselbert of Lorraine and, then, the Carolingian King Louis IV, and her sister Hadwidis married Hugh the Great, grandson of Robert the Strong. The kings of imperial Burgundy traced their ancestry back to Welf, who lived at the beginning of the ninth century and was the father of Louis the Pious's second wife Judith. A continuous succession of fathers and sons ruled Burgundy from 888, when Rudolph I was crowned, until 1032, when Rudolph III died without sons and the kingdom became part of the German kingdom. This family, too, intermarried with the other great families of the region: Richard le Justicier married the sister of Rudolph I, and King Conrad, son of Rudolph II, married the granddaughter of Henry the Fowler.³⁴

A final example is provided by the kings of Italy. The earliest known male

³² Richard may be the Richard, son of Childebrand, who appears in the Burgundian sources of the mid-ninth century as a member of a family related to the Carolingians; L. Levillain, "Les Nibelungen historiques et leurs alliances de famille," *Annales du Midi*, 49 (1937): 337-408, and 50 (1938): 5-66. Also see Maurice Prou and Alexandre Vidier, eds., *Recueil des chartes de l'abbaye de Saint-Benoît-sur-Loire*, 1 (Paris, 1900): 23-24, 59-67, nos. 9, 25. Another source, however, calls Richard "brother of Boson" and thus the son of a somewhat mysterious figure named Bovin; *Annales Bertiniani*, 882 (ed. Grat et al., 247). For the careers of Richard and his three sons, see the *Chronica S. Benigni* (ed. Bougaud and Garnier, 115, 118-19); Flodoard, *Annales*, 921, 923, ed. Ph. Lauer, *Les Annales de Flodoard* (Paris, 1905), 5, 8-9, 12-13; *Annales S. Benigni Divionensis*, 899, in *MGH, Scriptores*, 5: 40; Maximilien Quantin, ed., *Cartulaire général de l'Yonne*, 1 (Auxerre, 1854): 132-33, nos. 67-68; H. d'Arbois de Jubainville, *Histoire des ducs et des comtes de Champagne*, 1 (Paris, 1859): 450, no. 17; *Gallia Christiana*, 4: 67, no. 28; and André Déléage, ed., *Recueil des actes du prieuré de Saint-Symphorien d'Autun de 696 à 1300* (Autun, 1936), 19-24, nos. 6-7.

³³ Regino of Prüm, *Chronicon*, 818 (ed. Kurze, 73); *Annales Bertiniani*, 834 (ed. Grat et al., 15); Alberic de Trois-Fontaines, *Chronica*, 924, in *MGH, Scriptores*, 23: 757; and Werner, "Untersuchungen zur Frühzeit des französischen Fürstentums" (1960), 87-115.

³⁴ For the family of Henry the Fowler, see Liudprand of Cremona, *Antapodosis*, 2.20, 4.15, ed. Joseph Becker (1915), *MGH, Scriptores in usum scholarum*, 46-47, 112-13; Alberic de Trois-Fontaines, *Chronica*, 937, in *MGH, Scriptores*, 23: 761; and the genealogies in the eleventh-century *Chronicon Wirzburgense* of Ekkehard, *ibid.*, 6: 32. The charters of the kings of Burgundy have recently been edited; see Theodor Schieffer, *Die Urkunden der burgundischen Rudolfinger* (Munich [MGH], 1977); and, for the family's origins, see Josef Fleckenstein, "Über die Herkunft der Welfen und ihre Anfänge in Süddeutschland," in Gerd Tellenbach, ed., *Studien und Vorarbeiten zur Geschichte des grossfränkischen und frühdeutschen Adels* (Freiburg, 1957), 71-136. For Rudolph's ascension to the throne of Burgundy in 888, see René Poupardin, *Le Royaume de Bourgogne (888-1038): Étude sur les origines du royaume d'Arles* (Paris, 1907), 10-28. For the marriage between Adelaide, sister of Rudolph I, and Richard le Justicier, see Auguste Bernard and Alexandre Bruel, eds., *Recueil des chartes de l'abbaye de Cluny*, 6 vols. (Paris, 1876-1903), 1: 358-61, no. 379. [Hereafter, documents from Cluny will be cited in the form C 379, followed by the volume and page number of the Bernard-Bruel edition.]

ancestor is Evrard of Frioul, who married the daughter of Louis the Pious (by Judith, the daughter of Welf). Their son Berengar I and Berengar's grandson Berengar II were for much of the ninth and tenth centuries kings or claimants to the throne of Italy. The daughter of Berengar II married in succession the count of Flanders and King Robert II of France. When Berengar II, who was ruling with his son Adalbert II, was deposed by the emperor Otto I in 962, Adalbert fled to Burgundy, where he married the daughter of the count of Chalon and became the ancestor of the eleventh- and twelfth-century counts of Burgundy.³⁵

AS MEN FROM NEWER LINEAGES EMERGED FROM OBSCURITY, they sought marriages with daughters of the old nobility, of which the above six families are representative. If the ninth century is the period when the greatest families of northern France and the Empire appear, the tenth is the century of the counts. The comital families appear serving the great nobles who once served the Carolingians. If not actually "humble" in status when they first appear in the documents, the earliest known members of these tenth-century lineages were inferior to and dependent on members of families whose lineages had emerged a century earlier. The question of whether these families can truly be considered to be members of a "new" nobility requires a detailed history of their origins and early marriages. The founders of many comital families had their start as viscounts. Viscounts were dependent retainers of the counts, originally little more than bailiffs whom the counts established as their representatives when their possessions were too vast to be ruled personally.³⁶ Yet in the course of the tenth century a number of families with viscomital origins rose into the nobility and took the title of count.

A good example is the family of the counts of Anjou, who began their rise to power serving the heirs of Robert the Strong (who had become count of Anjou under the Carolingian Charles the Bald). When Count Fulk Réchin of Anjou composed the genealogy of his ancestors in the late eleventh century, he traced his line back to one Ingelgarius, the father of the first viscount, who lived at the very end of the ninth century; contemporary documents confirm Ingelgarius's existence.³⁷ His son Fulk the Red first began to consolidate the family's power in

³⁵ For Evrard of Frioul and his descendants, see Ignace de Coussemaker, ed., *Cartulaire de l'abbaye de Cysoing* (Lille, 1883), 1-5, 11, nos. 1, 6; the "Translatio S. Calixti Cisionium," in *MGH, Scriptores*, 15: 419; Raoul Glaber, *Historia*, 3. 2, ed. Maurice Prou (Paris, 1886), 56-57; Regino of Prüm, *Chronicon*, 888 (ed. Kurze, 129); and Adalbert, "Continuatio Reginonis," 952 (ed. Kurze, 166). Also see Edouard Favre, "La Famille d'Evrard, marquis de Frioul," *Études d'histoire du moyen âge dédiées à Gabriel Monod* (Paris, 1896), 157-62; Philip Grierson, "La Maison d'Evrard de Frioul," *Revue du nord*, 24 (1936): 241-66; and Eduard Hlawitschka, *Franken, Alemannen, Bayern und Burgunder in Oberitalien (776-962)* (Freiburg, 1960), 169-72. Evrard was probably the son of one Unroch, but nothing is known of him but his name.

³⁶ For the relatively low status of viscounts, see Martindale, "The French Aristocracy in the Early Middle Ages," 33-34. Beech has found that, in early medieval Poitou, counts but not viscounts were called *domini*; "Prosopography," 168.

³⁷ Fulk Réchin's short but invaluable "Fragmentum historiae Andegavensis" was edited by Halphen and Poupardin, as was the twelfth-century *Chronica de gestis consulum Andegavorum*; *Chroniques des comtes d'Anjou*, 232-38, 25-44. Also see Bertrand de Broussillon, ed., *Cartulaire de l'abbaye de Saint-Aubin d'Angers*, 1 (Paris, 1903); and Ch. Urseau, ed., *Cartulaire noir de la cathédrale d'Angers* (Paris, 1908); these two cartularies contain a number of

western France, being made viscount of Anjou by the Capetians.³⁸ As viscount, he should have exercised no power independent of the counts, but around 930 Fulk exchanged the title of *vicecomes* for *comes* in his charters, and the county of Anjou ceased to be a Capetian possession. (No stickler for niceties, Fulk had already made himself abbot of St.-Aubin of Angers, a practice not uncommon among the great nobles of the tenth century.) Fulk's grandson, Count Geoffrey Grey mantle, said in a charter of 966 that he held the county of Anjou "by the grace of God and of Duke Hugh" Capet, but the Capetians, who were distracted by their wars with the Vikings, the Carolingians, and the other great nobles of France, never recovered the county.³⁹ Indeed, at the end of the eleventh century, Count Fulk Réchin declared that his ancestors had received Anjou not from the Capetians but directly from the descendants of Charles the Bald; the Angevin counts had established their own authority.⁴⁰

Even while holding a title no more exalted than that of viscount, the ancestors of these counts began to marry into the ruling families of their region. According to the twelfth-century *Chronica de gestis consulum*, Ingelgarius married the niece of two noble bishops of the Orléanais and received Amboise from her uncles at the time of the marriage. According to the same source, Ingelgarius's son Fulk the Red married an *uxor nobilis* from whose paternal grandfather the bishops had originally received the lordship of Amboise.⁴¹ After Ingelgarius and Fulk the Red had married into this closely knit noble group of the lower Loire, Fulk's son and heir, Fulk the Good, married a woman of unknown origins, but his son Geoffrey Grey mantle married Adela, daughter of Robert of Vermandois and, thus, a direct descendant of Charlemagne. Geoffrey's daughter Gerberge married the count of Angoulême, the direct descendant of men who had been counts of Angoulême since the mid-ninth century. Geoffrey's sister Adelaide-Blanche married in succession Prince Raymond of Gothia (whose family had

tenth-century documents. For the classic study on the counts of Anjou, see Louis Halphen, *Le Comté d'Anjou au XI^e siècle* (Paris, 1906); also see his *Étude sur les chroniques des comtes d'Anjou et des seigneurs d'Amboise* (Paris, 1906). Still useful is Émile Mabille, "Introduction aux chroniques des comtes d'Anjou," in Paul Marchegay and A. Salmon, eds., *Chroniques des comtes d'Anjou* (Paris, 1871); Halphen relied on this work for many points. For more recent works on the counts of Anjou, see Werner, "Untersuchungen zur Frühzeit des französischen Fürstentums" (1958), 264–79; and Olivier Guillot, *Le Comte d'Anjou et son entourage au XI^e siècle*, 1 (Paris, 1972): vii–viii. Guillot has accepted without question Werner's work on the origins of the counts.

³⁸ Fulk appears as the viscount of Robert I around the year 900; Émile Mabille, ed., "Les Invasions normandes dans la Loire et les pérégrinations du corps de Saint Martin: Pièces justificatives," *Bibliothèque de l'École des Chartes*, 30 (1869): 440–45, nos. 8–9.

³⁹ Bertrand de Broussillon, ed., *Cartulaire de St.-Aubin*, 4–7, 59–61, 203–04, nos. 2, 36, 177. Werner has re-examined the documents to substantiate the fact that Fulk took the title "count of Anjou" only in 929 or 930, rather than being called both count and viscount of Anjou for two decades before 930, as had been previously thought; "Untersuchungen zur Frühzeit des französischen Fürstentums" (1958), 264–66.

⁴⁰ The fact that the Capetian king Philip I had recently stolen Fulk's wife doubtless added to the force of Fulk's insistence that his ancestors had received the county "non a genere impii Philippi"; "Fragmentum historiae Andegavensis" (ed. Halphen and Poupardin, 233).

⁴¹ *Chronica de gestis consulum Andegavorum* (ed. Halphen and Poupardin, 30–32). Werner gives a family tree showing the marriages of Ingelgarius and Fulk; "Untersuchungen zur Frühzeit des französischen Fürstentums" (1958), 277 n. 105. He has in error made the lord of Loches, who gave Amboise to the bishops, the maternal rather than the paternal grandfather of Fulk's wife, but in his discussion, he correctly followed the *Chronica*; *ibid.*, 275. It is not clear why he made the bishops' father the lord of Amboise, since he stated—following the *Chronica*—that they did not obtain Amboise from their father but rather from the above-mentioned lord of Loches.

been princes in southern France for at least a century), the Carolingian king Louis V (although she divorced him before he ascended the throne), and Count William I of Arles. She and Count William were parents of Queen Constance, third wife of King Robert II. After William's death, she may have been married a fourth time to Otto-William, Count of Burgundy, the son of King Adalbert II of Italy.⁴² In three generations, the upward mobility of the descendants of Ingelgarius had produced connections with some of the most important families of the older nobility, and his great-granddaughter was considered a suitable wife by at least three princes.

A comparable case of upward social mobility in a viscomital family is found in the counts of Chalon-sur-Saône. They, like the counts of Anjou, were originally delegated a county to hold in trust for members of the older nobility. But they quickly took power for themselves and, once possessed of the comital title, began to marry members of the class of their former lords. The earliest known member of the family is Robert, viscount of Dijon in the first half of the century. Dijon and Chalon were both at that time in the possession of Hugh the Black, one of the sons of Duke Richard le Justicier of Burgundy.⁴³ But about the middle of the tenth century, Lambert, one of Viscount Robert's sons, became count of Chalon. Neither the date nor the process by which he became count is precisely known. Lambert first appears in the documents concerned with property at Chalon in 944, although he is not specifically called "count" then. A charter drawn up about 980, near the time of his death, said that he obtained the comital office "assentante rege primoribusque Franciae." He may simply have appropriated Chalon and been accepted as count by the Carolingian king Louis IV or his young son Lothair at a time when the Carolingians were deeply involved in wars with the Capetians and the sons of Hugh the Black.⁴⁴

⁴² For Adela of Vermandois, see the "Genealogiae comitum Andagavensium" (ed. Halphen and Poupardin, *Chroniques des comtes d'Anjou*, 248, no. 2). For Gerberge's marriage with Count William of Angoulême, see Adémar de Chabannes, *Chronicon*, 3. 41, ed. Jules Chavanon (Paris, 1897), 163. Count William was the great-great-grandson of Count Vulgrin, whom Adémar (rather vaguely) called the *propinquus* of Charles the Bald; *ibid.*, 3. 19 (ed. Chavanon, 137). For the three marriages of Adelaide-Blanche, see Richer, *Historia*, 3. 92–95 (ed. Latouche, 2: 116–20). Although Richer stated quite explicitly that her first marriage was to Raymond of Gothia, Ferdinand Lot and all scholars after him concluded that this statement is an error, because the contemporary chronicler Liudprand called Raymond's wife Bertha; Lot, *Les Derniers Carolingiens* (Paris, 1891), 367; Liudprand, *Antapodosis*, 5. 31 (ed. Becker, 149). Lot decided that she must instead have married Stephen of Gévaudan, who appears in the documents with a wife named Adelaide. I see no reason to doubt Richer; even if Raymond did have his children by one Bertha, there is no reason why he should not have married Adelaide after Bertha's death. The documents attest that Otto-William's second wife was named Adelaide, and Pope Benedict VIII specifically referred to her in a letter of 1016 as "Adelaidis comitissa, cognomento Blancha"; Letter 16, *Patrologia Latina*, 139: 1603. The son of Countess Adelaide-Blanche of Arles had married the daughter of Otto-William, and it is not unlikely that she and Otto-William, both widowed, should have married at the same time as their children.

⁴³ Chalon is often confused by non-French scholars with Châlons-sur-Marne, a city 160 miles away. This family can best be traced through the charters it gave the family foundation of Paray; Ulysse Chevalier, ed., *Cartulaire de Paray-le-monial* (Paris, 1890). A history of the family has been written by J. Louis Bazin, *Les Comtes héréditaires de Chalon-sur-Saône (880–1234)*, Mémoires de la Société d'histoire et d'archéologie de Chalon-sur-Saône, ser. 2, vol. 4 (Chalon, 1911). But Bazin was rather vague about the tenth-century members of this family and made several slips in his genealogy of eleventh-century members, as indicated below. Viscount Robert appears as a viscount of Hugh the Black in a charter of 936; Déleage, *Recueil des actes de Saint-Symphorien*, 21–24, no. 7.

⁴⁴ C 655, 1: 609–10; and Chevalier, *Cartulaire de Paray*, 2–3, no. 2. Although some modern scholars have assigned either Lambert's wife or his mother to old families of the upper nobility in order to justify his acquiring Chalon, there is no evidence of such a link. Bazin made Lambert's wife the niece of Hugh the Black; *Les Comtes de Chalon*, 12.

However he obtained his office, Lambert was able to keep it. The son and brother of viscounts, he made the office of count of Chalon a hereditary possession of his descendants. The transitional status of the family at this time is illustrated by the very different disposition of his two daughters in marriage. One, Mathilda, married the lord of Semur, only a castellan. His other daughter, Gerberge, first married Adalbert II, deposed king of Italy (and bore him the famous Otto-William), and then Duke Henry of Burgundy, brother of Hugh Capet.⁴⁵ Although none of Lambert's sons married (the only one to survive him was Hugh, Bishop of Auxerre [999–1039], who ruled as count during his lifetime and left the county to Mathilda's children on his death), the marriage of his daughter Gerberge indicates that his family was considered suitable by a nobility in search of marriage partners. The similarities between the upward mobility of the counts of Anjou and that of the counts of Chalon are emphasized by the marriage, after Lambert's death, of his wife Adelaide to a count of Anjou, Geoffrey Grey mantle, whose first wife, Adela of Vermandois, had recently died.⁴⁶ Acceptable marriage partners for the Capetian dukes, relatives of the counts of Chalon also considered it appropriate to marry daughters of castellans, lower in the ranks of the nobility, and thus provided a conduit through which new blood-relations could flow.

A third family that began as viscounts and ended by being linked with royalty as well as with castellans through its marriages is the family of the tenth-century counts of Mâcon. William the Pious, duke of Aquitaine at the beginning of the tenth century, was also count of Mâcon. But in the confusion over the succession to Aquitaine after his death—the duchy went first to his *nepos* William the Young and then to the counts of Poitou—control of Mâcon was appropriated by William's viscounts. The first of the new line to call himself count was Alberic, an adventurer from Narbonne who married the daughter of Viscount Raculph of Mâcon.⁴⁷ This Alberic may be the same as the Alberic who appears, in a charter drawn up by the bishop of Narbonne, as the son of the vis-

⁴⁵ Most modern scholars make Gerberge the daughter of a hypothetical earlier marriage of Lambert rather than the marriage that produced Mathilda and Lambert's heir Hugh. The only evidence for this seems to be that Gerberge is referred to as Hugh's *germana* rather than his *soror*; "Gesta pontificum Autissiodorensium," 49, ed. L.-M. Duru, *Bibliothèque historique de l'Yonne*, 1 (Auxerre, 1850): 387. Since, however, *germana* was used in the documents of the time to refer to a full sister at least as often as a half-sister, and especially since the author had only a few lines before said that Hugh had no *germanus frater*, although he is known to have had at least one half-brother (thus using *germanus* to mean a full brother), it seems an unnecessary complication to make Gerberge the daughter of an unknown first wife of Lambert. An attempt by some nineteenth-century historians, and followed by Bazin, to make Gerberge the daughter of someone other than Lambert (translating *germana* as cousin) was refuted by Poupardin; Bazin, *Les Comtes de Chalon*, 18–20; and Poupardin, *Le Royaume de Bourgogne*, 414–19.

⁴⁶ Adelaide and Geoffrey Grey mantle had one son, Maurice; Urseau, *Cartulaire de la cathédrale d'Angers*, 56–58, no. 25; Marcel and Paul Canat de Chizy, eds., *Cartulaire du prieuré de Saint-Marcel-les-Chalon* (Chalon, 1894), 11–12, no. 6; C 1474, 2: 528–29; Mabille, "Introduction aux chroniques des comtes d'Anjou," lxx; and Eugène Jarry, *Formation territoriale de la Bourgogne* (Paris, 1948), table 1. Bazin doubted that Adelaide married Geoffrey Grey mantle, thinking that her second husband must have been some other Geoffrey; *Les Comtes de Chalon*, 17. It would have been impossible for her to marry, as Bazin suggested, a Geoffrey who was the father of Geoffrey of Semur (her daughter Mathilda's husband), as Geoffrey of Semur was the son of a man named Joceran.

⁴⁷ M.-C. Ragut, ed., *Cartulaire de Saint-Vincent de Mâcon* (Mâcon, 1894), 6, no. 7. Viscount Raculph appears with the designation *vocatus comes* in a charter from the final years of the ninth century; *ibid.*, 169–70, no. 284. Apparently control of Mâcon was already slipping during William's lifetime. For the fullest account of the tenth-century counts of Mâcon, see Poupardin, *Le Royaume de Bourgogne*, 211–19, 234.

count of Narbonne, but there is no evidence that members of his family had ever served as other than stewards for the upper nobility.⁴⁸ He and his son and grandson, however, called themselves counts, and both son and grandson married into the high nobility. Alberic's son, Count Leotold, married the daughter of Count Manasses of Burgundy. Leotold's son, Count Alberic II, married Ermentrude, the granddaughter of Duke Gisibert of Lorraine and of Gerberge, the sister of King Otto I.⁴⁹ This family of counts of Mâcon ended in the male line with Alberic II, who died around 980; he was succeeded as count of Mâcon by Otto-William, who married his widow Ermentrude.⁵⁰ In the female line, Alberic's daughter Beatrix married the lord of Château-Landon, thus tying the counts of Mâcon to a castellan family of the region (Château-Landon is in the Gâtinais, between Burgundy and Paris) as well as to the counts of Anjou: Beatrix's son, Geoffrey of Château-Landon, married the daughter of Count Fulk Nerra and became the father of Counts Geoffrey Barbu and Fulk Réchin.⁵¹ Thus, the tenth-century counts of Mâcon, like those of Anjou and Chalon, were able to rise from the level of viscounts to become counts, able to produce marriage partners socially suitable for the already established nobility.

A fourth and final example of a comital family arising from humble origins in the tenth century is the family of the counts of Nevers. This family's history was

⁴⁸ C. Devic and J. Vaissete, eds., *Histoire général de Languedoc*, ed. Émile Mabille and Edward Barry, 4 (new ed., Toulouse, 1875), cols. 130–31, no. 38.

⁴⁹ C 432, 1: 420–21; Siegfried of Gorze, letter to Poppo of Stablo, ed. Wilhelm von Giesebrecht, *Geschichte der deutschen Kaiserzeit*, 2 (5th ed., Leipzig, 1885): 714–18; and Flodoard, *Annales* (ed. Lauer, 158–59). Ermentrude has been called "of Roucy" by all historians since Lot decided that her father, who is not named in any source, must have been Raynold of Roucy; *Les Derniers Carolingiens*, 10, 10 n. 5. Raynold of Roucy is known as a vassal of Louis IV, but it would be very surprising that the contemporary chroniclers did not mention his marriage with Louis IV's stepdaughter if he had indeed married her; Flodoard, *Annales*, 947, 949, 953, 954 (ed. Lauer, 106, 123, 136, 139). Lot's only reason to suggest that Ermentrude was the daughter of Raynold was that she named her own son Raynold. Ermentrude's great-grandfather Raynold (or Reginald) of Lorraine, however, may equally well be the source of the name. Moreover, the sources say that Raynold of Roucy's daughter (by an unnamed woman) married the count of Sens, whereas Ermentrude married the counts of Mâcon and Burgundy; *Historia Francorum Senonensis*, 999, in *MGH, Scriptores*, 9: 369. Bernard Guenée, studying the well-documented eleventh- and twelfth-century lords of Roucy, was struck by the fact that they seemed completely unaware of any relationship to the German kings and the counts of Burgundy, a relationship that requires the assumption that Ermentrude's mother married Raynold of Roucy; Guenée, "Les Généalogies entre l'histoire et la politique: La Fierté d'être capétien, en France, au Moyen Âge," *Annales*, 33 (1978): 456–60. Ermentrude's father must therefore be someone other than Raynold of Roucy, although exactly who remains unknown.

⁵⁰ For Otto-William's acquisition of Mâcon through this marriage, see Ragut, *Cartulaire de Mâcon*, 6, no. 7; and Poupardin, *Le Royaume de Bourgogne*, 414–19. Also see Constance B. Bouchard, "Laymen and Church Reform around the Year 1000: The Case of Otto-William, Count of Burgundy," *Journal of Medieval History*, 5 (1979): 1–2. Modern scholars have often given two sons to Alberic II and Ermentrude, named Alberic and Leotold; Poupardin, *Le Royaume de Bourgogne*, 219, 219 n. 2; and Chaume, *Origines du duché de Bourgogne*, 465. There is, however, no evidence for their existence. Poupardin cited a charter supposedly witnessed by them; but no one named Alberic is among the witnesses, and the Leotold in the witness list is not identified as the count's son; C 1291, 2: 368.

⁵¹ "Genealogiae comitum Andagavensium" (ed. Halphen and Poupardin, 249, no. 5). Fulk Réchin gave his parents as Geoffrey of Château-Landon and Ermengard, daughter of Fulk Nerra; "Fragmentum historiae Andagavensis" (*ibid.*, 232). Orderic Vitalis and the Chronicle of St.-Maxient, however, made Fulk the son of Alberic of Château-Landon rather than Geoffrey; Orderic Vitalis, *Historia ecclesiastica*, 3. 6, ed. Marjorie Chibnall, *The Ecclesiastical History of Orderic Vitalis*, 2 (Oxford, 1969): 304; and *Chronicon Sancti Maxentii Pictavensis*, 1060, ed. Paul Marchegay and Émile Mabille, *Chroniques des églises d'Anjou* (Paris, 1869), 402. Their statements are explained by the fact that Beatrix had two sons, Alberic and Geoffrey, who in succession were lords of Château-Landon; Orderic and the chronicler of Poitou simply confused the two brothers. See M. Guérard, ed., *Cartulaire de l'église Notre-Dame de Paris*, 1 (Paris, 1850): 326–27, no. 19.

written in the middle of the twelfth century by Hugh of Poitiers, a monk of Vézelay. The earliest ancestor mentioned in his "Historia" is Landric, nephew of Bishop Hildegard of Autun (d. 893). Hildegard, according to the chronicler, was an undistinguished Poitevin and a clerk at the royal court who had become bishop of Autun because a king of France—unnamed—realized that Hildegard would be very capable of recognizing evil, after spotting him one evening "playing with a certain young woman."⁵² The bishop and his nephew were present with Richard le Justicier at the siege of the "little castle" of Metz in the Nivernais, Hugh of Poitiers continued, and Landric fought so well that he was given the *castellum* for his own. Whatever the accuracy of this story, it scarcely suggests exalted origins for this comital family. Hugh of Poitiers said further that Landric married a woman from Anjou, but it is not clear whether he meant a relative of the counts of Anjou.

Landric's son Bodo continued as a petty castellan of the Nivernais, but Bodo's son Landric, who first appears in the documents in the second half of the tenth century, established the family's power. In a long and active life, he served Duke Henry of Burgundy, Hugh Capet's brother, appearing in his charters with the designation *gloriosus miles*, abetted King Robert II in his attempts to stay married to—or at least "in his little bed" with—his cousin Bertha, went to war against a number of noted people, attempted to reform Vézelay by the expedient of driving all the monks out and asking the abbot of Cluny to send him some new ones,⁵³ and married the daughter of Count Otto-William of Burgundy. He acquired the county of Nevers, according to Hugh of Poitiers, as a gift from the count of Burgundy; it seems most likely that he obtained both Nevers and Auxerre at the time he married Otto-William's daughter.⁵⁴ In the wars that broke out in 1003 between King Robert II and the great Burgundian nobles, Landric, in spite of the help he had given the king in the affair of Bertha, held Auxerre against him during a protracted siege. He made friends with Robert again before his death, however, and married his son Raynold to Robert's sister Adelaide.⁵⁵ With this marriage, Raynold (d. 1040) was confirmed by the

⁵² This unedifying story was omitted by early editors of the "Historia"; "Origo et historia brevis Nivernensium comitum," ed. R. B. C. Huygens, *Monumenta Vizeliacensis: Textes relatifs à l'histoire de l'abbaye de Vézelay*, Corpus Christianorum continuatio mediaevalis, vol. 42 (Turnhout, 1976), 235–39. Vézelay had been troubled by the attacks of the counts of Nevers for over a century. The history is thus not altogether flattering, although it seems quite accurate—most points can be checked against contemporary documents—and the author found several counts in the lineage to be sympathetic figures.

⁵³ René de Lespinasse, ed., *Cartulaire de Saint-Cyr de Nevers* (Nevers, 1916), 49–50, no. 23; "Rythmus Satiricus," ed. G.-A. Hücker, "Les Poèmes satiriques d'Adalbéron," in Achille Luchaire, ed., *Mélanges d'histoire du moyen-âge*, Bibliothèque de la Faculté des Lettres, vol. 13 (Paris, 1901), 82–86; *Annales Nivernenses*, in *MGH, Scriptores*, 13: 90; Richer, *Historia*, 4. 98 (ed. Latouche, 2: 310–12); *Miracula Sancti Benedicti*, 5. 15–16 (ed. de Certain, 212–13); and Quantin, *Cartulaire de l'Yonne*, 166–67, no. 87. On the "Rythmus Satiricus," also see Ferdinand Lot, "La Chanson de Landri," *Romania*, 32 (1903): 1–17.

⁵⁴ Both of these counties had been held by Robert the Strong; it seems likely that they had been inherited by his descendant, Duke Henry of Burgundy, who had in turn left them to his stepson, Otto-William. *Annales Bertiniani*, 865 (ed. Grat et al., 123).

⁵⁵ Raoul Glaber, *Historia*, 3. 2 (ed. Prou, 57). Also see the "Origo et historia brevis" (ed. Huygens, 238); Clarius of Sens, *Chronicon Sancti Petri Vivi Senonensis*, 1003, ed. Robert-Henri Bautier and Monique Gilles, *Chronique de Saint-Pierre-le-Vif de Sens, dite de Clarius* (Paris, 1979), 106; and Jean Richard, *Les Ducs de Bourgogne et la formation du duché du XI^e siècle* (Paris, 1954), 4. Raoul Glaber incorrectly made Adelaide the daughter rather than the sister of Robert II, a slip that has led several modern scholars astray; *Historia*, 4. 9 (ed. Prou, 113).

king in his possession of the counties of Auxerre and Nevers. His son William married Ermentrude, the heiress of the county of Tonnerre (her ancestors had held the county since the mid-tenth century); henceforth, the three counties of Auxerre, Tonnerre, and Nevers were inherited together by William's descendants. Landric's second son, Bodo, married the daughter of Count Fulk Nerra of Anjou.⁵⁶ Thus, during the course of the tenth century, Landric of Metz and his descendants had advanced from petty castellans to powerful counts, and their advance was given recognition when they were considered suitable marriage partners for the granddaughter of the last king of Italy and the daughter of the first king in the unbroken line of Capetian kings of France.

The counts of Anjou, Chalon, Mâcon, and Nevers are typical of families that began in the early tenth century as viscounts or castellans, serving the great lords of the older families, but that during the century obtained the comital office for themselves by their own strength: in part they were rewarded by the great lords for their service, and in part they took what they wanted while the great lords were distracted elsewhere. Many other examples could be added. The counts of Champagne traced their origins back only to Thibaud Tricheur, count of Blois and Chartres in the mid-tenth century, who seems to have been the son of a Viscount Thibaud, who had held his power in the name of the Capetians, although Thibaud Tricheur married a daughter of the Carolingian count of Vermandois.⁵⁷ The eleventh- and twelfth-century counts of Joigny and Joinville were descended from the viscounts of Sens, who had in the late tenth century begun to call themselves counts and marry daughters of local counts.⁵⁸

About the same time that they obtained their comital titles, the counts of all of these lineages began to marry women of high noble or royal blood. After a generation or two as counts, they began to marry their own daughters to men of royal blood. Even though some members of the old nobility insisted on marrying king's daughters themselves, in many cases noblemen in search of spouses did not distinguish between counts' daughters with long pedigrees and counts' daughters of relatively recent position. A pedigree consisting of two or three generations of powerful and highly respected men would do almost as well around the year 1000 as a longer pedigree of royal but weak ancestors. The eleventh-century historian Sigibert, though speaking of nobility as residing in a long ancestry, did not trace his hero Wicbert's pedigree back before his grandparents; in fact, he said that the power that came from a large number of living relatives

⁵⁶ Quantin, *Cartulaire de l'Yonne*, xxviii-xxx; Guillot, *Le Comte d'Anjou*, 27, 27 n. 139; and de Lespinasse, *Cartulaire de St.-Cyr*, 89, no. 47. His third son, Robert, was engaged to the daughter of the castellan of Gourdon, but he seems to have died before the marriage took place; Aimon of Fleury, *Miracula Sancti Benedicti*, 5. 16 (ed. de Certain, 213).

⁵⁷ André Chédeville, *Chartres et ses campagnes, XI^e-XIII^e siècles* (Paris, 1973), 252; Guy Devailly, *Le Berry du X^e siècle au milieu du XIII^e* (Paris, 1973), 129; and Michel Bur, *La Formation du comté de Champagne, v. 950-v. 1150* (Nancy, 1977), 151-53. Bur has suggested that Viscount Thibaud may have married a granddaughter of Charles the Bald. Whatever the accuracy of this suggestion, it is important to note that all of the exalted blood of the early counts of Blois and Chartres came from their maternal rather than paternal ancestry. Raoul Glaber said the counts came from an *obscura linea*; *Historia*, 3. 9 (ed. Prou, 86).

⁵⁸ *Historia Francorum Senonensis*, 999, in *MGH, Scriptores*, 9: 369; and Werner, "Untersuchungen zur Frühzeit des französischen Fürstentums" (1959), 172-73.

and the wealth that came from a multitude of possessions were what gave rise to worldly nobility.⁵⁹ The old nobles of the tenth century always seem to have considered nobility to reside (at least in theory) in the blood, but, when they were looking for spouses for their daughters, power and wealth—in a count with a short pedigree of powerful viscounts—made a man perfectly acceptable as a son-in-law. And, in choosing wives for themselves, they found women who had at least one female ancestor in the old nobility suitable.

AFTER THE YEAR 1000, the size of the nobility again expanded as a new group of families, the castellans, began to consolidate their power and marry the daughters of older noble families, as had the viscounts before them. By the eleventh century, the old nobility, enlarged by the inclusion of the former viscounts, had begun to lose part of its wealth due to generations of partible inheritance and of generous gifts to local monasteries.⁶⁰ At this time a new social group, the castellans, accumulated and consolidated sufficient power to join the noble ranks. The castles that had sprung up across Europe in the tenth and eleventh centuries in the wake of the invasions became central points around which power could be built; Robert Boutruche has even spoken of the advent of castles as one of the most significant influences on the society of the central Middle Ages. Men who had originally been appointed custodians of these new castles quickly became hereditary lords. They imitated the customs and manners of the old nobility and came to be considered noble and, thus, suitable partners for nobles in search of spouses.⁶¹ A few examples will illustrate the process.

The lords of Semur, mentioned above in connection with the counts of Chalon, first appeared as castellans with Geoffrey I, at the very end of the tenth century. Geoffrey's father Joceran seems to have owned a large amount of land in the Lyonnais; he and his brothers often made gifts to the abbey of Savigny, where his brother was abbot. No evidence suggests, however, that Joceran and his brothers were considered members of the nobility: they were never called *nobilis* in their charters and held no office or castle. But Geoffrey I acquired the

⁵⁹ Vita Wicberti, 1, in *MGH, Scriptores*, 8: 508: "Siquidem ex prosapia numerositate accrescebat potestatis eminentia, ex possessionum multiplicitate rerum affluebat opulentia, per quos cumulati et provehi solet mundanae nobilitatis potentia." Also see Génicot, "La Noblesse au moyen âge," 8.

⁶⁰ Georges Duby, *Guerriers et paysans, VII^e-XII^e siècles: Premier essor de l'économie européenne* (Paris, 1973), 187-92 (*The Early Growth of the European Economy*, trans. Howard B. Clarke [Ithaca, 1974], 167-71); and Martindale, "The French Aristocracy in the Early Middle Ages," 28.

⁶¹ Robert Boutruche, *Seigneurie et féodalité*, volume 2: *L'Apogée (XI^e-XIII^e siècles)* (Paris, 1970), 11; Georges Duby, "The Diffusion of Cultural Patterns in Feudal Society," *Past & Present*, no. 39 (1968): 5-8 (reprinted in his *Chivalrous Society*, 171-75), and *Les Trois Ordres ou l'imaginaire du féodalisme* (Paris, 1978), 188-89, 352-55 (*The Three Orders: Feudal Society Imagined*, trans. Arthur Goldhammer [Chicago, 1980], 151-52, 293-95); Sidney Painter, "Castellans of the Plain of Poitou in the Eleventh and Twelfth Centuries," *Speculum*, 31 (1956): 247-49; Marcel Garaud, *Les Châtelains de Poitou et l'avènement du régime féodal, XI^e et XII^e siècles*, *Mémoires de la Société des Antiquaires de l'Ouest*, vol. 8 (Poitiers, 1954), 15-27; Parisse, "La Noblesse Lorraine," 203-12; and Edouard Perry, "Les Châteaux du Roannais du XI^e au XIII^e siècle," *Cahiers de civilisation médiévale*, 9 (1966): 13-27. Both viscounts and castellans seem, in many areas, to have originated as dependent officers of the counts. But the two groups, although they often overlapped, should be distinguished; the castellans obtained their positions later and generally were not able to rise as high as the viscounts; see Bur, *La Formation du comté de Champagne*, 442-58.

castle of Semur and, about the same time, married the daughter of Count Lambert of Chalon, thus becoming the ancestor of the eleventh- and twelfth-century counts of Chalon. He was also married a second time, to an unknown woman by whom he had Damas I, who succeeded him as lord of Semur. The marriages of Damas's children indicate the extent to which this rather short castellan lineage had been accepted into the nobility, for his daughter Helias married Duke Robert I of Burgundy, grandson of Hugh Capet, and his son Geoffrey II married Adelaide, daughter of Count Raynold of Burgundy—himself son of Otto-William and grandson of the last king of Italy.⁶²

Another family that went from obscure castellan origins to marriages with counts and dukes is the family of the lords of Vignory. Their origins were definitely not in the nobility of Charlemagne's time, for, according to a brief history of the family drawn up in the twelfth century, the founder of the line was a Norseman named Raoul Barbeta.⁶³ The descendants of this tenth-century chieftain quickly established themselves as part of the castellan society of Burgundy. The origins of the wives of the first few lords of Vignory are unknown, but, in the late eleventh century, Gui IV married Beatrix, sister of the Capetian duke of Burgundy and of Bishop Robert of Langres, and Gui's sister married Count Roger of Joinville.⁶⁴ Thus, the descendants of a Norse chieftain had become respectable castellans in the course of the eleventh century, to the point that a lord of Vignory and his sister could marry into lineages powerful since the ninth or tenth century.

A final example of upward mobility is provided by the Grossi family of the Mâconnais. The earliest known male ancestor, Joceran, lived at the end of the tenth century. Since he did not have a castle he cannot be called a castellan, and he is given no noble titles in his charters, but he married a woman whose ancestors had been lords of Brancion and called *nobilis* since the early tenth century. But Joceran seems to have held a fairly large amount of allodial land in the region north of Cluny, and his descendants (who took the surname Grossus) acquired the castellanies of Uxelles and then of Brancion (from his wife's relatives). In the eleventh century, his male descendants married daughters of other petty allodists of the Mâconnais; but in the twelfth century one married the sister of the duke of Lorraine, and a son of this couple married the daughter of the count of Chalon.⁶⁵

⁶² See the detailed genealogy of the lords of Semur in Jean Richard, ed., *Le Cartulaire de Marcigny-sur-Loire* (Dijon, 1957). For Geoffrey I's father, see Bernard, *Cartulaire de Savigny*, 236, 268, 324–25, nos. 435, 527, 645.

⁶³ "Rudolphus Barbeta, Normannus," as cited in Ernest Petit, *Histoire des ducs de Bourgogne de la race capétienne*, 6 vols. (Dijon, 1885–98), 2: 434 n. 2. The history of the lords of Vignory has been studied by two modern scholars: Petit, *Histoire des ducs de Bourgogne*, 1: 304, and 2: 430–38; and J. d'Arbaumont, ed., *Cartulaire du prieuré de Saint-Étienne de Vignory* (Langres, 1882), lxxviii–ciii. These two scholars—apparently unaware of each other's work—differed only slightly in their reconstruction of the family tree. After re-examining their sources, I believe that there are two separate people, Gui II and Gui III, where they have only one, Gui II; and I see no reason to include Lambert, Bishop of Langres (1016–30), in the family tree. Their studies will, however, certainly serve for an introduction to the family, and their comments on the lords' marriages are fully attested in the documents.

⁶⁴ Jacques Laurent, ed., *Cartulaires de l'abbaye de Molesme*, 2 (Paris, 1911): 204, no. 1. 220. Alberic de Trois-Fontaines, *Chronica*, 1110, in *MGH, Scriptores*, 23: 818.

⁶⁵ For the Grossi, see Johannes Fechter, "Cluny, Adel und Volk" (Doctoral dissertation, University of Tü-

Examples could be multiplied. Members of a number of castellan families with no counts in their ancestry began in the late eleventh century to marry daughters of comital families and to marry their own daughters to counts. The process continued throughout the twelfth century. The effect was that, by the end of the twelfth century, the great noble families of counts and dukes were related by blood or marriage to a large proportion of the viscounts and castellans of their regions.⁶⁶

This recruitment of the castellans by the nobility, aside from demonstrating social mobility, is connected with a trend toward regionalism and localism. In the ninth century, the upper nobility had been "international." As Störmer discovered in his study of ninth- and tenth-century Bavaria, there was no purely "local" nobility at that time because all members of the noble class were related.⁶⁷ By the twelfth century, however, most counts and dukes had fewer far-flung possessions than the nobles of three centuries earlier. The castellans who had joined the nobility by the twelfth century and established their own wealth and power had never had much authority beyond their geographic regions. As a result, with a greater variety of local noble families from which to choose their spouses, the nobility of the twelfth century tended to marry closer to home. For example, the Capetian duke Hugh II of Burgundy (1102–43) had at least seven children who married (three additional sons became bishops and one daughter a nun), and all but one of these seven married someone of a comital or castellan family from in or near Burgundy.⁶⁸ None of these spouses came from lineages established before the tenth or eleventh century, but all were considered suitable partners for the children of a duke whose ancestors had been kings, dukes, and counts since the mid-ninth century.

This gradual broadening of the size of the nobility continued in the late twelfth and thirteenth centuries, as descendants of simple knights, the armed retainers of counts and castellans, began to enter the ranks of the nobility (in spite

bingen, 1966), 40–41; and Duby, *La Société mâconnaise*, 336–42. Duby stated that the Grossi issued from the old noble lineage of "Evrard," but he gave no evidence for this suggestion and, indeed, said that it would be foolhardy to try to trace Joceran's lineage before 950; *ibid.*, 336 n. 79. The family tree may be constructed in some detail from the charters attesting their generosity to Cluny. Also see Georges Duby, ed., *Recueil des pancartes de La Ferté-sur-Grosne, 1113–1178* (Paris, 1953).

⁶⁶ Léopold Génicot, studying a small area of Belgium, found that in the eleventh century most of the princes were allied by marriage with the lesser nobles who had no great ancestry. He considered these marriages an attempt by the princes to control the lesser nobles; whatever the purpose, the result was a group of closely related nobles of all ranks in Hainault and Namur. Génicot, "Noblesse et principautés en Lotharingie du XI^e au XIII^e siècle," in *Scrinium Lovaniense: Mélanges historiques/Historische opstellen Étienne van Cauwenbergh* (Louvain, 1961), 195–96.

⁶⁷ Störmer, *Adelsgruppen*, 190. Also see Wenskus, *Sächsischer Stammesadel*, 466–67.

⁶⁸ For Odo II's marriage with Marie of Champagne (daughter of the count), see Jubainville, *Histoire de Champagne*, 3: 464, no. 144. For the marriage of Hugh Rufus with Isabel of Chalon, see Petit, *Histoire des ducs de Bourgogne*, 2: 346, no. 516, and 3: 517, no. 1500. For Raymond's marriage with Agnes of Grignon, see *ibid.*, 2: 268–69, nos. 355–56. For Clemence's marriage with a lord of Donzy (probably Geoffrey III), see Jean Richard, "Sur les alliances des ducs de Bourgogne aux XII^e et XIII^e siècles," *Annales de Bourgogne*, 30 (1958): 41 n. 4. For Mathilda's marriage with Count William of Montpellier, see Luc d'Achery, *Spicilegium*, ed. Étienne Baluze and Edmund Martène, 3 (new ed., Paris, 1723): 526. For Sybil's marriage with King Roger of Sicily (the one marriage far outside the region of Burgundy), see Romoald, *Annales*, 1148, in *MGH, Scriptores*, 19: 425. For an (unnamed) daughter's marriage with Hugh of Vaudemont, see Alberic de Trois-Fontaines, *Chronica*, 1161, *ibid.*, 23: 845.

of the increasing resistance of the already established nobles to accept new additions to their ranks).⁶⁹ Knights first appear in French records during the wars and strife of the late tenth century, and they became common by the end of the eleventh century. These dependent warriors were well on their way by the end of the twelfth century to becoming part of the nobility.⁷⁰

THUS, THE NEW NOBLES, whether they began as viscounts, castellans, or knights, quickly entered the web of alliances that had united the members of the old nobility with each other. Although counts and dukes did not marry the daughters of knights, by the twelfth century the lords of smaller castles often married knights' daughters. When these smaller castellans married their own daughters in turn to more powerful castellans and the more powerful castellans married their daughters to viscounts and counts, their knightly blood entered comital lineages. Members of this web of interrelated aristocratic families could say that they were characterized by wealth, power, and birth. The wealth and power were undoubted, in both the new and the old nobility, and members of the new nobility could generally also pass on the criterion of birth. Although written "proofs" of nobility were not, of course, required before the thirteenth century, most counts of the eleventh century (even if descended very recently from viscounts in the paternal line) as well as many castellans in the twelfth century could point to mothers or grandmothers who came from old noble families: the newly established nobles acquired "old" noble ancestors through their mothers, and, equally significant, "old" nobles who married the daughters of newly established counts (who themselves had married "old" noble women), after they had been established for a few generations, bestowed "new" ancestry on their own children.

This understanding of a closely related nobility, into which new lineages quickly became integrated, explains the paradox of "new" versus "old" nobility in medieval social structures. On the one hand, men of the twelfth century believed that "new" men had quite recently entered the ranks of the nobility. On the other, most of the "new" men—or *their immediate descendants*—can be demon-

⁶⁹ Lucien Musset has detailed the emergence of the new Norman nobility in stages during the tenth and eleventh centuries; the counts were often bastard relatives of the dukes, but the viscounts were apparently unrelated. The castellans were of local French—rather than Scandinavian—stock, and knights were of quite humble origins. Musset, "L'Aristocratie normande au XI^e siècle," 75–77, 90, and "Aux origines d'une classe dirigeante: Les Tosny," *Francia*, 5 (1977): 45–53.

⁷⁰ For the emergence of the concept of *miles*, see Georges Duby, "La Diffusion du titre chevaleresque sur le versant méditerranéen de la Chrétienté latine," in Contamine, *La Noblesse au moyen âge*, 42–52. Although the scholarly consensus is that knights were not originally part of the nobility, it is clear that they came to be considered nobles in the late twelfth and thirteenth centuries, as the concept of knighthood was exalted and all aristocrats became increasingly subject to royal authority; Duby, "La Noblesse dans la France médiévale," 159–60 ("The Nobility in Medieval France," 108–09); Génicot, *Les Hommes—La Noblesse*, 121–35; and Parisse, "La Noblesse Lorraine," 226, 300. The German *ministeriales* also experienced upward mobility in the twelfth century, although they did not rise as high as the free *milites* of France; Freed, "The Formation of the Salzburg Ministerialage," 95–102. Werner, though denying that there was anything "new" about the tenth- and eleventh-century counts, called the rise of knights into the ranks of the nobility "revolutionary"; "Important Noble Families in the Kingdom of Charlemagne," 180.

strated to have had in their ancestry at least one member of the old nobility, a pattern of descent that has caused many modern scholars to declare that there was no "new" nobility at all. An especially striking example of this paradox is that most of the important lineages of twelfth-century France had some Carolingian blood—that is, Charlemagne can be placed in their ancestry by modern scholars—yet they themselves seemed neither to know nor to care about this ancestry. When Hugh Capet replaced the last French Carolingian, Louis V, all contemporaries agreed that Hugh was not of the blood of Charlemagne; but his grandmother Beatrix was the daughter of the count of Vermandois, a descendant in the direct male line from Charlemagne. Although this connection between the second and third "races" of kings of France was noted in the later Middle Ages and has been confirmed by modern scholars, it was apparently unrecognized in the tenth and eleventh centuries.⁷¹ Indeed, at the end of the twelfth century, Louis VII's marriage with Adela of Champagne and Philip II's with Isabel of Hainault were each hailed by Philip's supporters as at last creating a union between the Carolingian and Capetian lines; the earlier connections were unknown.⁷²

This apparent paradox can be resolved by realizing that the men of the Middle Ages could not be acutely aware of every ancestor, especially since distant blood connections with old royal families were not the chief determinants of the assumption of royal power. Hugh Capet seems to have relied on his election by the French nobility and the strength of his supporters rather than on any Carolingian blood to justify his assumption of the throne. At a time when the last Carolingians appeared helpless against the invasions, which were resisted by the Capetians in France (the Vikings), the Ottonians in Germany (the Magyars), and Berengar and his descendants in Italy (the Magyars and the Arabs), practical power rather than any distant link with Charlemagne determined the candidates when the aristocracy met to choose a new king.⁷³ The

⁷¹ See Hugh's letter of 931, in which he mentioned his mother Beatrix; *Recueil des historiens des Gaules et de la France*, 9: 719. She was the daughter of the count of Vermandois; see the "Chronicon Sithiensis," *ibid.*, 77; the *Historia Francorum Senonensis*, in *MGH, Scriptores*, 9: 366; and Orderic Vitalis, *Historia ecclesiastica*, 7. 1 (ed. Chibnall, 4: 345). For the descent of the counts of Vermandois from Charlemagne, see note 33, page 513, above. In spite of this evidence, Hugh Capet's accession was considered in the eleventh and twelfth centuries to have ended the line of Charlemagne's descendants on the throne. See, for example, the eleventh-century *Historia Francorum Senonensis*, "Hic deficit regnum Karoli Magni," in *MGH, Scriptores*, 9: 368; and Adémar de Chabannes, *Chronicon*, 3. 30 (ed. Chavanon, 151). Adémar approvingly described Hugh's election as king, replacing a family that had lost the favor of God by neglecting the churches with the Capetian line. Also see Karl Ferdinand Werner, "Die Nachkommen Karls des Grossen bis um des Jahr 1000 (1.–8. Generation)," in Wolfgang Braunfels and Percy Ernst Schramm, eds., *Das Nachleben*, volume 4 of *Karl der Grosse, Lebenswerk und Nachleben* (Düsseldorf, 1967), 413.

⁷² The thirteenth century saw the development of an elaborate myth in which Philip II's son Louis VIII represented the final union of the Carolingian and Capetian lineages. See Karl Ferdinand Werner, "Die Legitimität der Kapetinger und die Entstehung des 'Reditus regni Francorum ad stirpem Karoli,'" *Die Welt als Geschichte*, 12 (1952): 203–25; Gabrielle M. Spiegel, "The *Reditus Regni ad Stirpem Karoli Magni*: A New Look," *French Historical Studies*, 7 (1971): 145–74; and Andrew W. Lewis, "Dynastic Structures and Capetian Throne-Right: The Views of Giles of Paris," *Traditio*, 33 (1977): 225–48.

⁷³ For the election of the first Capetian kings of France, Odo and his brother Robert I, see Abbo, *Bella Parisiacae urbis*, 2. 442–48, ed. Henri Waquet, *Le Siège de Paris par les normands* (Paris, 1942), 98–100; and Flodoard, *Annales*, 922 (ed. Lauer, 10). For the ascension of Berengar I in Italy, see Liudprand, *Antapodosis*, 1. 15, 2. 33, 3. 1 (ed. Becker, 17, 53, 73).

earliest genealogies that attempted to show the descent of comital families from the Carolingians were composed in the late eleventh century, although these comital families had often held office from the ninth or tenth century.⁷⁴ Not until the twelfth century was any general effort made to link the new kings and counts of Europe with those of the eighth century.

But there is more to the fact that apparent "new" men were unaware of the old nobles in their family trees than simple forgetfulness or a lack of concern for such niceties at a time when a strong sword arm was of chief importance. Also vital is that the old noble lineages came to be part of the pedigrees of the "new" nobles primarily through marriage. Emil Kimpen has given the example of the Ottonian and Salian kings of Germany, who in the male line had rather undistinguished ancestors but who had for a long time married women who may have had Carolingian blood (in their own maternal ancestry).⁷⁵ Nobles who could trace their lineage back to the tenth century generally concentrated on the first male in their paternal line to attain the comital title—that is, the first "new" noble who was given or appropriated power. When Fulk Réchin wrote the history of the counts of Anjou at the end of the eleventh century, for example, he gave most of his attention to the counts, starting with Ingelgarius, not to their wives. By emphasizing the first viscounts in the male line to become counts, men of the eleventh and twelfth centuries were certainly justified in speaking of "new" nobles.

If their ancestry is traced strictly in the male line (or, at least, in the line that held a certain office or county), as was done by the noble families themselves, the nobles of the eleventh century clearly had tremendously diverse origins. Families emerged from obscurity as somewhat amorphous collections of relatives and became restructured as conscious groups identified by land or office and a male lineage, to use Schmid's concepts discussed above.⁷⁶ Modern scholars who take "old" nobility in the maternal ancestry of a recently established lineage to prove that they are not "new" are emphasizing ancestors about whom eleventh- and twelfth-century nobles were usually ignorant. It would be more useful to ask whether the first male of a lineage to acquire office was already a member of a powerful noble family. Since the greatest European families had achieved this "restructuring" of family consciousness by the end of the ninth century, the conclusion would seem warranted that those lineages with ancestry

⁷⁴ Léopold Génicot, "Princes territoriaux et sang carolingien: La *Genealogia comitum Buloniensium*," in *Études sur les principautés lotharingiennes*, Recueil de travaux d'histoire et de philologie, ser. 6, vol. 7 (Louvain, 1975), 264, 279, 289. Although the late eleventh-century genealogy of the counts of Boulogne showed that they were descended—through women—from Charlemagne, this same text stated that Robert II "non fuit de styrpe illa." Also see Werner, "Die Legitimität der Kapetinger," 208–10; and Parisse, "La Noblesse Lorraine," 233.

⁷⁵ Emil Kimpen, "Zur Königsgenealogie der Karolinger- bis Stauferzeit," *Zeitschrift für die Geschichte des Oberrheins*, 103 (1955): 57–63. Adalbold, the biographer of Henry II (1002–24), boasted that the king was descended from Charlemagne in seventeen generations on his father's side and sixteen on his mother's; Adalbold, "Vita Henrici II imperatoris," in *MGH, Scriptores*, 4: 684. Although Kimpen's genealogical tables, attempting to demonstrate how seventeen generations could be fitted into 200 years, are quite tentative, it is clear that Henry II's Carolingian blood, if he had any, came through women. Also see Léopold Génicot, "Recent Research on the Medieval Nobility," in Reuter, *The Medieval Nobility*, 20–21.

⁷⁶ See page 504, and note 11, above, esp. Schmid, "The Structure of the Nobility in the Earlier Middle Ages," 39, 49.

only traceable to the tenth and eleventh centuries did not have paternal ancestors among the great ninth-century noble families. But the medieval nobility appears to have always been composed of both old and new elements. A family's entry into the aristocracy was recognized when its members took spouses from the upper nobility. Once the noble marriage had been made, of course, all descendants of an upwardly mobile man had some ancestors of high noble blood. In attempting to prove that there was no "new" nobility, some modern scholars have mistaken the effects of acceptance into the nobility—that is, exalted ancestors for one's children—for the cause.

Thus Werner, followed by several other historians, believed that, by demonstrating "biological continuity" between ninth- and twelfth-century families, he had proved that there was "no new nobility."⁷⁷ Yet most of these families considered themselves the heirs of a man who had seized a comital office, though not a member of the old aristocracy. Werner's example of the marriages of the early counts of Anjou shows, at most, that men without exalted origins could become marriage partners for the daughters of the old nobility. Though he rejected as "fables" the ancestors Tortulf and Tertulle given in the twelfth-century history of the counts of Anjou as the grandfather and father of Ingelgarius (the first historical personage in the history), Ingelgarius certainly had *some* paternal ancestors. Whoever they were, they could not have been members of the small group of nobles into which Ingelgarius and his son Fulk Rufus married. Even if one accepts Werner's suggestion that Adelhard, grandfather of Fulk's wife, was the son or nephew of the Adelhard who served as seneschal for Louis I, an assumption that rests only on the similarity of names, it does not follow that Ingelgarius and Fulk themselves belonged to the small elite of royal attendants. Fulk was most likely *not* a descendant of Louis I's seneschal if he married that seneschal's great-granddaughter.⁷⁸ On the basis of Ingelgarius's and Fulk's marriages, Werner concluded that there was no "feudal anarchy" or "social revolution," but the evidence could equally well suggest that the old, inbred court elite was casting its nets wide for new spouses and that something like a "social revolution" was actually taking place.

Duby encountered a similar difficulty in constructing pedigrees of petty castellans of the Mâconnais in order to prove their Carolingian origins. An examination of the links he found between the castellans and the "old" nobility shows that they are nearly always through a daughter (or daughter's daughter) or a fe-

⁷⁷ Werner, "Untersuchungen zur Frühzeit des französischen Fürstentums" (1958), 256, 263; (1959), 182; (1960), 119. For analyses that have accepted his views, see Martindale, "The French Aristocracy in the Early Middle Ages," 10, n. 20; Beech, "Prosopography," 164; Guillot, *Le Comte d'Anjou*, vii-viii; Warlop, *The Flemish Nobility before 1300*, 43-52; Bur, *La Formation du comté de Champagne*, 98; and Boussard, "L'Origine des familles seigneuriales," 306.

⁷⁸ Werner, "Untersuchungen zur Frühzeit des französischen Fürstentums" (1958), 279. Similarly, I do not find it necessary to accept his conclusion that Fulk's wife's grandmother was the daughter of some great count named Gui only because Fulk named one of his sons Gui. Werner has not been clear about which Count Gui this might be, saying rather that there was a large and important family of "Widonen." Tellenbach has, however, cast into doubt the existence of such a family, saying that, although there were a number of men named Gui in the ninth and tenth centuries, there is no evidence that more than a few were related; "Zur Erforschung des hochmittelalterlichen Adels," 326.

male cousin. It can thus be argued that he has established not that these castellans were descended from younger sons of the old aristocracy or that they represent the restructuring of a large, old noble family into narrow lineages but rather that they were descended from upstart castellans who had acquired enough power to be considered suitable sons-in-law by noble fathers with a large number of eligible daughters. Similarly, Boussard postulated that the seigneurial lineages of the Loire must be the (male line) descendants of the "less favored" cadet branches of the ninth-century nobility, but, when he studied individual families in detail, his evidence shows not less favored noble sons but rather men like Thibaud, viscount of Blois and ancestor of the counts of Champagne. Boussard freely admitted that Thibaud may have been a "soldier of fortune" who married the daughter of the count of Vermandois. This alliance does not prove that this family had "Carolingian" origins, however, but merely that it had origins in an upwardly mobile man who caught the eye of the old nobility.⁷⁹

It should now be clear that mere demonstrations of "biological continuity" do not prove that the nobility was a closed caste from the ninth century onward.⁸⁰ Rather, men of new rich and powerful lineages became part of the closely related web of noble families by marrying daughters from older families and marrying their own daughters to other nobles.⁸¹ The answer to the question of whether or not there was a "new" nobility in Western Europe in the tenth and eleventh centuries thus depends on the definition of "new." If the presence of an "old" noble in a family's ancestry is considered sufficient to justify calling it an "old" family, then, as many modern scholars have claimed, there could have been no "new" nobility at all. But one would be equally justified in calling *all* of the eleventh- and twelfth-century nobles the products of "new" families, since they all had at least one castellan, soldier of fortune, or viscount in their ancestry.⁸²

⁷⁹ Duby, *La Société mâconnaise*, 318–19; and Boussard, "L'Origine des familles seigneuriales," 309–11. It seems unlikely that the variety of social and economic position among eleventh- and twelfth-century noble families was, as Boussard has suggested, purely a matter of younger noble sons being less "fortunate." As Karl Schmid has pointed out, early medieval families were very concerned with the welfare of living brothers and sisters; "Neue Quellen zur Verständnis des Adels im 10. Jahrhundert," *Zeitschrift für die Geschichte des Oberrheins*, 108 (1960): 185–232. With this concern for all of their children and with the enormous wealth that ninth-century noble families could distribute, it is difficult to see how some younger sons could be "less favored." The great ninth-century families about which detailed knowledge of their children is possible all provided very well for them. (Actually, the marriage with the daughter of the count of Vermandois seems to have been made by Viscount Thibaud's son, rather than the viscount himself, but this does not change the argument; see page 520, note 57, above.)

⁸⁰ For arguments against a "closed noble caste," also see Tellenbach, "Zur Erforschung des hochmittelalterlichen Adels," 321–22; Tabacco, "Su nobilità e cavalleria," 5, 22; and Timothy Reuter, "Introduction," in *The Medieval Nobility*, 7.

⁸¹ Few scholars besides Léo Verriest believe that *nobilitas* was transmitted strictly in the female line; see his *Noblesse, Chevalerie, Lignage* (Brussels, 1959). His confusion seems to come from the fact that a twelfth-century woman might be called *nobilis mulier* while her husband was called *dominus* or (by the late twelfth century) *miles*, in either case without the adjective *nobilis*. In fact, *dominus* was an indication of nobility by itself; a *mulier* would be qualified as *nobilis* only to indicate her status if her title did not convey this—there was for example no feminine form of *miles* for a knight's wife. See Duby, "La Noblesse dans la France médiévale," 151 ("The Nobility in Medieval France," 98–99); Beech, "Prosopography," 168; Martindale, "The French Aristocracy in the Early Middle Ages," 70; and Warlop, *The Flemish Nobility before 1300*, 16.

⁸² For a discussion of the process of mixing of blood lines, which continued through the late medieval period

As a glance at Figures 1 and 2 shows, in the late eleventh century Count Fulk Réchin of Anjou could have found in his family tree a social spectrum ranging from castellans of Château-Landon through viscounts of Narbonne and counts of Mâcon to Charlemagne's immediate descendants. The dukes of Aquitaine of the same time could have traced their ancestry to dukes of Saxony, kings of Italy, and Charlemagne, but also to viscounts of Dijon. The contemporary counts of Nevers had among their ancestors the same viscounts of Dijon, dukes of Saxony, and Carolingians as the dukes of Aquitaine had, and the counts of Nevers had, in addition, castellans of Metz. But, when the contemporary histories of these families were written, the focus was invariably on the line of males who had held the comital or ducal office. Granted, attainment of noble status was often signaled by marrying a woman of an old family, but beyond the preceding one or two generations nobles were primarily aware of their male ancestors.

The attempt by modern scholars to demonstrate "biological continuity" has primarily been intended to establish that the tenth century, a period that has seemed to merit the title "Dark Ages" more than any other in the Middle Ages, was not the turning point that historians have often assumed it to be—was not a time of rupture between the old Carolingian and new "feudal" nobility.⁸³ It is not, however, necessary to rehabilitate the tenth century completely in order to dispute that old idea. Certainly the invasions, wars, and confusion created an atmosphere particularly suitable for the rise of new men. Yet this sudden appearance of new counts in the records does not mean that all of the old counts were gone, for some were certainly still in evidence; when a new count appears in the records, he is generally seen allied with—or at war against—the Capetians, the counts of Vermandois, the dukes of Aquitaine, or the descendants of Richard le Justicier, to say nothing of the Carolingians, all of whom had roots in the ninth century or earlier. New men had, of course, been on the rise since long before the tenth century. The *senatores* of sixth-century Gaul included many new men among their numbers, and the Carolingians themselves before they rose to authority and the crown in the eighth century were just one of almost a dozen Frankish ducal families of approximately equal power and ambition.⁸⁴ The period between the ninth and twelfth centuries was certainly a period when the nobility expanded, but this need not mean it was a time of "rupture."

It is not necessary, moreover, to argue that the ninth-century nobles, a small group, were the sole ancestors of the much more numerous twelfth-century aristocracy in order to show continuities from the early to the High Middle Ages.⁸⁵

and into modern times, see Anthony Richard Wagner, *English Genealogy* (Oxford, 1960), 178–209. Descendants of early modern kings may be found in all social classes, and modern kings themselves may have ancestors in all social classes.

⁸³ Boussard has denied the "rupture" of the tenth century most explicitly; "L'Origine des familles seigneuriales," 321. A similar attempt is behind the work of Werner and all who have followed him.

⁸⁴ Frank D. Gilliard, "The Senators of Sixth-Century Gaul," *Speculum*, 54 (1979): 695–96; Archibald R. Lewis, "The Dukes in the Regnum Francorum, A.D. 550–751," *Speculum*, 51 (1976): 406–08; and Werner, "Important Noble Families in the Kingdom of Charlemagne," 147.

⁸⁵ Boussard has even suggested that the ninth-century nobility must have been very fecund; "L'Origine des familles seigneuriales," 322.

As Schmid has pointed out, it is hard to trace *any* descendants for the majority of Charlemagne's contemporaries, much less to find all later nobles descended from them. As is well known, some important lineages died out in the eleventh and twelfth centuries; it would be difficult to assert that such extinctions did not take place in the violent ninth and tenth centuries.⁸⁶ Rather, those ninth-century families who continued into the tenth were joined by new noble families, into which they married, and together these two groups of families were joined in status and marriage with the castellans of the eleventh century.

TWO QUESTIONS REMAIN after examining the nobility as an expanding web of interrelated lineages. First, why would members of the old aristocracy be willing to marry their daughters to new men—and after a few generations be willing to marry women of new lineages themselves? And, second, if the new nobles were not just unrecognized descendants of old families, then who were they? Answers here must remain tentative, but some suggestions are possible.

Surely one reason why the new nobles were able to marry into older lineages so readily was pragmatic: if a noble had a large number of marriageable daughters, he would consider a rich and powerful count—though of relatively short pedigree—as a logical choice for one of them. Duby's work has shown the economic advantages that accrued to medieval families in marrying off their daughters, advantages that facilitated marriages to relative upstarts.⁸⁷ But there are also some indications that, from the tenth century onward, old noble families were making a conscious search for spouses from lineages to which they were not already related. Since, during the ninth century, the small number of families that constituted the old aristocracy had all intermarried, in the tenth century they either had to find new sources for marriage partners or else had to marry their cousins. And marriages between cousins were forbidden, from the late ninth century, by the church's stand against broadly defined "incest." Limitations of space do not permit a full discussion here of this point, but it should at least be noted now that, in the tenth and eleventh centuries, the period during which viscounts and castellans were welcomed as marriage partners for counts' daughters, *very* few marriages occurred between persons who were less closely related than fourth or fifth cousins—and these usually ended in divorce. Only in the twelfth century, when generations of intermarriage had once again made it very difficult to find a social equal who was not also one's cousin, did nobles begin entering into marriages in the full knowledge that they were technically incestuous.⁸⁸

⁸⁶ Schmid, "Zur Problematik von Familie," 2; and Bouchard, "The Structure of a Twelfth-Century French Family," 50–51.

⁸⁷ Duby, "Le Mariage dans la société du haut moyen âge," 30–31.

⁸⁸ Constance B. Bouchard, "Consanguinity and Noble Marriages in the Tenth and Eleventh Centuries," *Speculum*, 56 (1981): 268–87. Flagrant flouting of the decrees led to doubts of the validity of the ecclesiastical definition of consanguinity in the late twelfth century and eventually to a reduction of the "forbidden degrees." See John W. Baldwin, *Masters, Princes, and Merchants: The Social Views of Peter the Chanter and His Circle*, 1 (Princeton, 1970): 333–36; and Duby, *Medieval Marriage*, 80.

Before then, the solution was frequently to choose a partner from a lineage that was not already related to the present nobility because it sprang from non-noble ancestors. As the first of the line to appear was generally a man who was made a dependent viscount or castellan by the well-established nobility, yet who was quickly able to make himself independent, it seems logical to conclude that the old counts chose their "representatives" from the nonnoble free men of the region. The existence of such a class of free men is well attested. French scholars have long since given up the thesis that nobility and freedom were exact equivalents in the Middle Ages.⁸⁹ The ninth- and tenth-century documents from St.-Bénigne and Cluny (two houses with unusually rich archives from before 1000) are filled with the names of men who appear without comital or ducal title, without the designation *nobilis*, being generally called only *quidam homo*, yet with enough land to make sizeable gifts to monasteries.⁹⁰ The first ancestors of the lords of Semur and Uxelles, for example, seem to have been of this class. Free men often had allods even if they were not noble. Although at one time scholars thought allodial property was unusual in the "feudal" period, a number of regional studies have revealed that it was actually quite common. The frequency of allods is masked only by the fact that a given parcel rarely appears in the documents except when it is given to a church or a feudal lord and thus loses its allodial character.⁹¹

Free men, with enough wealth in allodial property to release them from the necessity of engaging in agriculture themselves, would have made ideal candidates for the nobility to choose as viscounts or castellans.⁹² The fragmentary evidence from the ninth century indicates a variety in personal status; the conscious distinction between the nobles, "those who fight," and the peasants, with no one in between, did not come until the eleventh century (if then).⁹³ One is thus not faced with a stringent choice of either making the new nobility descendants of peasants or making them all descendants of the very small group of

⁸⁹ Duby, *La Société mâconnaise*, 192, and *Les Trois Ordres*, 187 (*The Three Orders*, 150). Also see Giles Constable, "Monasticism, Lordship, and Society in the Twelfth-Century Hesbaye: Five Documents on the Foundation of the Cluniac Priory of Berrée," *Traditio*, 33 (1977): 188–93. Werner has shown that a large group of free men existed in the ninth through eleventh centuries, even though he declared that they must have kept their place as vassals of the nobility since there was "no room for upstarts" in noble ranks; "Untersuchungen zur Frühzeit des französischen Fürstentums" (1959), 186.

⁹⁰ Bernard and Bruel, *Chartes de Cluny*, vols. 1–3; and Joseph Garnier, ed., "Chartes bourguignonnes inédites des IX^e, X^e, et XI^e siècles," *Mémoires de l'Académie des inscriptions et belles lettres de l'Institut national de France*, new ser., 2 (1849): 1–168.

⁹¹ Regional historians have generally concluded that the frequency of allods in the region under study makes it different from the rest of France; the number of such studies from many different parts of France with the same conclusions suggests rather that the "prevailing wisdom" on the frequency of allods needs to be changed. These studies include Robert Boutruche, *Une Société provinciale en lutte contre le régime féodal: L'Alleeu en Bordelais et en Bazadais du XI^e au XVIII^e siècle* (Rodez, 1947); Archibald R. Lewis, *The Development of Southern French and Catalan Society* (Austin, 1966); Elizabeth Magnou-Nortier, *La Société laïque et l'église dans la province ecclésiastique de Narbonne (zone cispyrénéenne) de la fin du VIII^e à la fin du XI^e siècle* (Toulouse, 1974); and Duby, *La Société mâconnaise*. Bur has suggested that in the eleventh century in Champagne allods were at least half as frequent as benefices and could be held by members of all levels of society; *La Formation du comté de Champagne*, 395.

⁹² The petty castellans of the Mâconnais, whom Duby studied, seem to have originally held their property as allods, when they became linked by marriage near the beginning of their histories with the older noble families; *La Société mâconnaise*, 142–50.

⁹³ Duby, *Les Trois Ordres*, 18–19, 209 (*The Three Orders*, 7–8, 169).

ninth-century nobles. Freemen with a little wealth, though not yet a castle or county, were available to fill any gap and to marry any unattached noble woman. Although the scanty evidence makes any firm conclusion impossible, it is attractive to postulate that viscounts and castellans who entered the ranks of the nobility in the tenth and eleventh centuries had free and ambitious—though nonnoble—possessors of allodial property in their backgrounds.

ALL ATTEMPTS TO DEFINE THE NOBILITY of the High Middle Ages as either “new” or “old” have so far been overly simplistic. To insist on the tenth and eleventh centuries as a period of a sharp break in the composition of the class is to ignore the many indices of biological continuity and also the continuity in the very concept of *nobilitas*. Yet this continuity does not mean that the group was frozen. As old lineages died out, new ones appeared, and, as the remaining old nobility sought nonconsanguineous alliances, all of them soon shared an ancestry composed of both soldiers of fortune and Carolingian counts through their marriages with each other’s daughters. Although modern scholars of institutions and government may prefer to emphasize the continuities in the ruling aristocracy, medieval men saw themselves as living in a period of mobility and opportunity: a new noble pointed with pride to the first ancestor who had made himself part of the ruling group rather than to the marriage with an older line that had set the seal on his success.

The Black Death and Western European Eschatological Mentalities

ROBERT E. LERNER

AS THERE IS a "Richter scale" for measuring earthquakes, so there is now a "Foster scale" for measuring disasters. Harold D. Foster, a Canadian geographer, has maintained that disasters ought not to be ranked solely by their toll in lives but also by the physical damage and emotional stress they create. By these standards the Black Death of 1347 to 1350 falls two-tenths of a point short of being the worst disaster in history: on the Foster scale, World War II (11.1) ranks first, the Black Death (10.9) second, and World War I (10.5) third.¹

Without stopping to quibble about the two-tenths of a point, or to ask whether one can calibrate emotional stress so finely, we might agree that the Black Death was one of the worst disasters on record. Among the numerous vivid illustrations of the horror are a German chronicler's image of ships floating with dead crews aimlessly on the seas and an Italian chronicler's offhand hyperbole that "there was not a dog left pissing on the wall."² Granted that the disaster was enormous, the question to be asked here is, how was it placed within the framework of eschatological thought? At a time when people believed seriously in the end of the world and the Last Judgment, how did they place the onslaught of the greatest disaster yet known within their conception of the history of salvation?

Obviously, even during the greatest of disasters, not everybody reacts in the same way. Robert Benchley once remarked that in every news photo of epoch-making events there always seems to be a man in a derby hat looking in the opposite direction from the action: on Bloody Sunday in St. Petersburg or assassination day in Sarajevo, a "Johnny-on-the-spot" is always looking up at a clock,

This essay was first conceived as a paper delivered at a conference on the Black Death held by the Center for Medieval and Early Renaissance Studies of the State University of New York, Binghamton, October 1977. I have benefited from criticism by Charles M. Radding and from review of my transcriptions by Daniel Wiliman. Research expenses were generously funded by the National Endowment for the Humanities, the American Council of Learned Societies, and Northwestern University. Abbreviations used in the footnotes include the following: BL—British Library, London; BN—Bibliothèque Nationale, Paris; MGH—*Monumenta Germaniae Historica*; and Töpfer, *Reich des Friedens*—Bernhard Töpfer, *Das kommende Reich des Friedens* (Berlin, 1964).

¹ Foster, "Assessing Disaster Magnitude," *Professional Geographer*, 28 (1976): 241–47.

² Mathias von Neuenburg, *Chronica*, ed. A. Hofmeister, in MGH, *Scriptores rerum Germanicarum*, new ser., 4: 263–64; and *La Vita di Cola di Rienzo*, ed. A. M. Ghisalberti, bk. 2 (Rome, 1928): chap. 3, as translated in John Wright, *The Life of Cola di Rienzo* (Toronto, 1975), 103.

picking his teeth, or waving insouciantly at the camera. Quite apart from those who lived in parts of Europe where the Black Death did not strike, there were probably some people in 1348 or 1349 who went about their business or pleasure in the eye of the storm. They may have given no thought to the end of the world or the events that might precede it and would have waved at the camera had there been one to wave at. Hence, I do not mean to argue that everyone in Europe, when confronted by the plague, thought about the history of salvation. (It would be good to know how many did and how many did not, but unfortunately we never can.) What I do mean to argue is that the onslaught of disaster did lead many to wonder about how it fit into God's plan, and the ways in which they did so are of considerable interest to the cultural historian.

Indisputably, many in Western Europe took the plague to be an eschatological sign. The Arabic chronicler as-Sulūk reported that Christians on Cyprus who experienced the Black Death "feared that it was the end of the world."³ As-Sulūk probably misunderstood their fear somewhat, because no medieval Christian believed that the world would end without certain culminating events—such as the reign of Antichrist—preceding the Last Judgment. But, certainly, many thought that the Black Death signaled God's displeasure and in some way presaged the End. A Franciscan chronicler of Lübeck, for example, wrote that the Black Death was a divine punishment for human evil and a sign of the last days, qualifying this assessment with the assurance that exactly when those days would come God alone knew. Similarly, the Swiss Franciscan, John of Winterthur, claimed that a great earthquake of 1348 and the plague (which had not yet hit his own region) were antecedents of the terrible disasters that the Lord had warned would come before his Second Coming (Matthew 24: 7; Luke 21: 11).⁴

Although helpful to start with, such statements not only leave open the question of how long it would be before the End would arrive—respecting Christ's injunction in Acts 1: 7 that "it is not for you to know the times or the seasons which the Father hath put in his own power"—but also say nothing about what might happen between the time of the plague and the Last Judgment. Yet people surely speculated on just that point. What might their thoughts have been?

REMARKABLY LITTLE RESEARCH has been done on this subject, perhaps because scholars have been unaware of evidence that would help them treat it. Hitherto, the only approach has been through the sensational—the macabre processions

³ As-Sulūk, as quoted in Michael W. Dols, *The Black Death in the Middle East* (Princeton, 1977), 290.

⁴ *Detmar-Chronik*, in C. Hegel, ed., *Die Chroniken der deutschen Städte vom 14. bis in 16. Jahrhundert* (Leipzig, etc., 1862–), 19: 522: "... so sint desse stervende, orloghe, vorretnisse unde al de plaghe, de nū scheen, mer de tekene, de Cristus heft ghesproken in den hilgen ewangelien, dat se scholen scheen vor der lesten tiid; wo langhe vore, dat is nicht beschreven, wente Gode is dat alleneghen bekant"; and Johannes Vitoduranus, *Chronica*, ed. F. Baethgen, in *MGH, Scriptores rerum Germanicarum*, new ser., 3: 276: "Predicta, scilicet terre motus et pestilencia, precurrencia mala sunt extreme voraginis et tempestatis secundum verbum salvatoris in ewangelio dicentis: 'Erunt terre motus per loca et pestilencia et fames' et cetera." Also see an attack of 1348 against speculations about the birth of Antichrist in the unpublished *Monastica* of Conrad of Megenberg, as quoted in Sabine Krüger, "Krise der Zeit als Ursache der Pest?" in *Festschrift für Hermann Heimpel*, 2 (Göttingen, 1972): 857.



Figure 1: A flagellant procession at the time of the Black Death. Illustration taken from the *Konstanzer Weltchronik*, third quarter of the fifteenth century, Bavaria; Munich, Bayerische Staatsbibliothek, MS Cgm 426, f. 42v.

of the flagellants. In the nineteenth century Herman Haupt connected the flagellant processions with eschatological prophecies and maintained that “the flagellants felt called upon to prepare the way for the coming kingdom of God.” Dilating on this theme, Norman Cohn has written that the German flagellants of 1349 were “eschatologically inspired hordes” whose activities “ended as a militant and bloody pursuit of the Millennium.” Like Cohn, Philip Ziegler stated in a popular book on the Black Death that the German flagellants conceived of their movement ending “only with the redemption of Christendom and the arrival of the Millenium [*sic*].” Similarly, the East German scholar Martin Erbstößer, limiting his argument to the flagellants of Thuringia and Franconia (who he believed were more radical than the others), argued that the flagellants “felt themselves to be the proclaimers of a new time, that of the preparation for the end of the world.”⁵

⁵ Haupt, “Kirchliche Geisselung und Geisslerbruderschaften,” in *Realencyklopädie für protestantische Theologie und Kirche*, 6 (Leipzig, 1899): 437; Cohn, *The Pursuit of the Millennium* (1957; 3d ed., New York, 1970), 136–39; Ziegler, *The Black Death* (1969; reprint ed., New York, 1971), 92; and Erbstößer, *Sozialreligiöse Strömungen im späten Mittelalter* (Berlin, 1970), 32. Karl Müller had already criticized a first attempt by Haupt to demonstrate flagellant millenarianism; Müller, “Die Arbeiten zur Kirchengeschichte des 14. und 15. Jahrhunderts,” *Zeitschrift für Kirchengeschichte*, 7 (1885): 113–14.

The flagellants, then, it is commonly assumed, were millenarians. I also think it likely that they were, but I must concede with Richard A. Kieckhefer that the assumption is based on very slender evidence. Putting aside the later "cryptoflagellants," whose millenarianism is not in doubt, only two sources speak in any way of flagellant millenarianism, and neither of these presents incontrovertible proof.⁶ The first and somewhat more detailed of these sources is an increasingly familiar friend of the heresiologist known as the Breslau manuscript. The anonymous author of a theological *questio* in this fifteenth-century manuscript attributed to the flagellants an eschatological song, which stated that in seventeen years after 1349, after many tribulations, the religious orders, particularly the mendicants, would expire, to be replaced by a new one. After that, the old orders would be restored "with great glory" and then the world would end.⁷

One problem with this report is chronological, for the theological *questio* postdates the appearance of the Hussites in the early fifteenth century. As Kieckhefer has noted, even if the author thought he was writing about the flagellants of 1349, he may have confused their ideas with those of the cryptoflagellants or attributed to them a song that they had not sung.⁸ That may be hypercritical: perhaps the flagellants of 1349 did sing about great changes coming within seventeen years. But the song in question still does not show them to be the eschatological radicals portrayed by Cohn, Ziegler, and Erbstösser. Cohn in particular appears to have distorted the evidence by interpreting the song as, "of course, . . . a prophecy in the Joachite tradition." According to him, "it is certain that when [the] flagellants talked of a new monastic order of unique holiness they were referring to themselves alone."⁹ But, in fact, the flagellants in the song never say this. Moreover, the text does not pose a clear opposition between the old orders and the new one—that is, a "Joachite" opposition between an old and a new dispensation—but instead foretells the ultimate glorious restoration of the old orders. In short, the passage says nothing about a revolutionary role for the flagellants; it merely foresees a time of tribulations followed by one of "great glory" before the End.

⁶ Kieckhefer, "Radical Tendencies in the Flagellant Movement of the Mid-Fourteenth Century," *Journal of Medieval and Renaissance Studies*, 4 (1974): 157–76, at 167–69. Kieckhefer has convincingly argued for the exclusion of other texts that have previously been adduced to demonstrate the alleged millenarianism of the flagellants. Siegfried Hoyer's claim that the flagellants of 1349 "followed the directives of a prophet" rests on a mistranslation of a fifteenth-century Dutch text, as in Paul Fredericq, *Corpus documentorum inquisitionis haereticae pravitatis Neerlandicae*, 1 (Ghent, 1899): 197; see Hoyer, "Die thüringische Kryptoflagellantenbewegung im 15. Jahrhundert," *Jahrbuch für Regionalgeschichte*, 2 (1967): 148–74, at 161.

⁷ The *questio* from the Breslau manuscript remains unedited, but Erbstösser has published the part concerning the flagellants; *Sozialreligiöse Strömungen im späten Mittelalter*, 27–28 n. 82. The relevant section reads "Item de quadam sua cantilena dicebant quod post 17 annos immediate presentem annum domini 1349 sequentes religiones et precipue mendicantium ordines post multas [Erbstösser: "multis"] tribulationes deficient substituto quodam novo ordine. Postquam etiam priores ordines cum magna gloria resuscitabuntur et tunc mundus certissime finietur. . . ."

⁸ Kieckhefer, "Radical Tendencies in the Flagellant Movement," 167–69. Erbstösser speculated that remarks about the Hussites might have been added later but gave no grounds for concluding that they were; *Sozialreligiöse Strömungen im späten Mittelalter*, 27.

⁹ Cohn, *The Pursuit of the Millennium*, 137. Erbstösser has called attention to another of Cohn's mistakes—namely, the assumption that, since the *questio* is found in a manuscript now in Breslau, it must have pertained to flagellants who were there; in fact, the manuscript was almost certainly copied in Erfurt; *Sozialreligiöse Strömungen im späten Mittelalter*, 26.

The other source that seems to contain some evidence of flagellant eschatological hopes is the "heavenly letter" reportedly read aloud during a flagellant sermon in Strassburg. This text, supposedly brought to earth by an angel, tells of Christ's anger with rampant impiety, warns of great punishments, and calls for reform and penance. But it also promises that, if men were to change their ways, Christ's anger would be assuaged and a time of blessedness and fruitfulness for the earth would ensue.¹⁰ Aside from the vagueness of this promise, the difficulty with the passage for assessing flagellant views is that it did not originate with the flagellants. The heavenly letter was a text put forward repeatedly in different versions throughout the Middle Ages, and the part in question is known to have been formulated by about 1200 at the latest.¹¹ If the flagellants paid any particular attention to it, how central it was to their own beliefs is still impossible to know.

Cohn called the heavenly letter the "manifesto of the flagellant movement." Whether or not there is any truth in this designation, the letter was certainly not a manifesto for revolutionary millenarian action, for the clear and simple message of the text read in its entirety is the call to "repent and be saved."¹² Whatever the flagellants' ideas about the future, there is little question that the driving motivation behind their processions was not to "pursue the millennium" but to do penance in the hope of appeasing God's wrath and thereby warding off the plague. If the flagellants were millenarians (and, as I said, I think they were), they were to that degree not unusual but typical of their age. In my view, most who thought about the significance of the plague were millenarians or chiliasmists. Inasmuch as this may be controversial, the rest of this essay will be devoted to explaining and supporting my contention.

ALTHOUGH THE WORD "chiliasm" has taken on for some the connotation of being more extreme than millenarianism, I consider the two words to be synonymous because both have the same etymology—coming respectively from the Greek and Latin for one thousand. In what follows, I use chiliasm instead of millenarianism simply because it is less cumbersome. Along with most scholars, I do not limit chiliasm to belief in a literal thousand-year kingdom ruled over by Christ but define the term more broadly to mean the expectation of imminent, supernaturally inspired, radical betterment on earth before the Last Judgment. Where I differ from some scholars is in my belief that chiliasm does not have to be oriented toward revolutionary action, although it certainly can be. Rather, it

¹⁰ Fritsche Closener, *Strassburgische Chronik*, in Hegel, *Die Chroniken der deutschen Städte*, 8: 114: "So wil ich uber ouch dän minen heiligen segnen, so bringet daz ertrich frucht mit gnoden und würt alle die welt erfüllet mit miner wirdekeit." Kieckhefer has found a lack of millenarianism in the heavenly letter, but he did not deal explicitly with this passage; "Radical Tendencies in the Flagellant Movement," 168.

¹¹ Erbstößer, *Sozialreligiöse Strömungen im späten Mittelalter*, 46, n. 157, citing the research of R. Priebsch. Bernhard Töpfer stated succinctly that the apparently chiliasmist passage in this heavenly letter represents "recht unbestimmter Hoffnungen . . . ein ausgeprägt joachimitisches Gepräge zeigen diese Erwartungen allerdings nicht"; Töpfer, *Reich des Friedens*, 282–83.

¹² Gordon Leff, *Heresy in the Later Middle Ages: The Relation of Heterodoxy to Dissent, c. 1250–c. 1450*, 2 (New York, 1967): 488–89.



Figure 2: Antichrist calls up devils to take over the world, and Archangel Michael descends to smite him. Reproduced from *Buch von dem Entkrist*, colored woodcuts, mid-fifteenth century. (Photograph provided by Professor Gerald Strauss, Indiana University.)

can also encourage perseverance in the face of persecution and bring hope in the face of trials.¹³

¹³ On the definition of chiliasm (or millenarianism) I follow, among many others, Norman Cohn, "Medieval Millenarism," in Sylvia L. Thrupp, ed., *Millennial Dreams in Action* (New York, 1970), 31; Y. Talmon, "Millenarian Movements," *European Journal of Sociology*, 7 (1966): 200, as cited in Clarke Garrett, *Respectable Folly: Millenarians and the French Revolution in France and England* (Baltimore, 1975), 1; and Michael Barkun, *Disaster and the Millennium* (New Haven, 1974), 18. Earlier scholarship, particularly under Cohn's influence, was primarily interested in finding a chiliasm that expresses "an active desire to speed the inevitable result, often through violent, revolutionary means"; Barkun, *Disaster and the Millennium*, 18. In the last few years, however, some scholars have been paying more attention to chiliasm's passive or conservative face. Bryan W. Ball, for example, has emphasized that "a millenarian was not, *ipso facto*, a heretic or even necessarily an extremist"; Ball, *A Great Expectation: Eschatological Thought in English Protestantism to 1660* (Leiden, 1975), 233. For an excellent bibliographical review that points to a trend away from the preoccupation with revolutionary chiliasm and assumptions of psychopathology, see Hillel Schwartz, "The End of the Beginning: Millenarian Studies, 1969-1975," *Religious Studies Review*, 2, no. 3 (1976): 1-15 (kindly called to my attention by Professor Kieckhefer). Bernard McGinn, in an anthology that appeared after this paper was written, has preferred the word apoca-



Figure 3: Devils lead Antichrist to Hell, with the representation of fire and brimstone. Reproduced from *Buch von dem Entkrüst*, colored woodcuts, mid-fifteenth century. (Photograph provided by Professor Gerald Strauss, Indiana University.)

There were two main varieties of chiliasm in the Middle Ages: the “post-Antichrist” and the “pre-Antichrist” strains.¹⁴ The former was more grounded in tra-

lypticism to millenarianism or chiliasm but agreed that “beliefs about the coming age . . . were as important for social continuity as they were for social change . . . , as often designed to maintain the political, social, and economic order as to overthrow it”; McGinn, *Visions of the End: Apocalyptic Traditions in the Middle Ages* (New York, 1979), 28–36, at 30.

¹⁴ The terminology is my own. I developed the distinction, expanding on the work of Marjorie Reeves’s *The Influence of Prophecy in the Later Middle Ages* (Oxford, 1969), in my “Refreshment of the Saints: The Time after Antichrist as a Station for Earthly Progress in Medieval Thought,” *Traditio*, 32 (1976): 97–144. Documented support for what I say in this and the following two paragraphs appears in that article. “Post-” and “pre-Antichrist” should not be confused with “post-” and “premillennial” terms that refer to debates among early modern and modern millenarian theologians about whether the second advent of Christ would come after or before the millennium. Medieval “pre-Antichrist” chiliasm did not address itself to the pre- or postmillennial question at all, and medieval “post-Antichrist” theologians differed as to whether the millennium to come after Antichrist would be brought in directly by Christ’s second advent or not.

ditional biblical exegesis than the latter and, therefore, was more often expressed in formal treatises by identifiable writers. Building upon agreement in the standard early medieval biblical commentaries of St. Jerome on Daniel 12: 11–12, the Venerable Bede on Revelation 8: 1, and Haimo of Auxerre on the Pauline epistles (I Thessalonians 5: 3 and II Thessalonians 2: 8) that there would be a period of intermission on earth between the demise of Antichrist and the Last Judgment, numerous twelfth-century writers independently expressed varieties of post-Antichrist chiliasm. Whereas the early medieval authorities abhorred chiliasm and only allowed the idea of a final time on earth because Scripture seemed to offer no alternative, such thoroughly orthodox twelfth-century commentators as Honorius Augustodunensis, Otto of Freising, Hildegard of Bingen, Gerhoch of Reichersberg, and the anonymous author of the *Glossa ordinaria* on Daniel welcomed the idea of a final time after Antichrist and made it serve variously for the “refreshment of the saints,” the conversion of the heathen and the Jews, and the reformation and purification of the Church. In Gerhoch of Reichersberg’s view, the time after Antichrist would be one of “great joy for the people of God.”

The theory of a wondrous time on earth after the death of Antichrist was brought to its first full flowering at the end of the twelfth century in the luxuriantly fertile prophetic writings of the Calabrian abbot, Joachim of Fiore. Joachim granted the last time the dignity of being a full historical age by making it serve concurrently as the seventh age of the Church, the seventh age of the world, and the third “status” of historical progress typified by the unfolding of the Trinity. In addition, Joachim was the first medieval exegete to read the most explicitly chiliastic passage in the Bible, the prediction of the reign of the saints with Christ on earth in Revelation 20, as alluding to the time after Antichrist. Finally, Joachim broadened the positive conception of the final time still wider than did other twelfth-century writers: for him it would be marked not just by profound peace and the conversion of unbelievers but also by the highest possible levels of ecclesiastical organization and human spiritual illumination short of Paradise.

Owing to its firm anchoring in standard biblical exegesis and to the gathering momentum of its elaborations, the idea of a wondrous time on earth after Antichrist became a virtually unquestioned assumption of Western eschatological theology from 1200 until the end of the Middle Ages. To take only one example, a “best-selling” theological handbook designed as a convenient reference work for the use of clerics, Hugh Ripelin’s *Compendium theologiae veritatis* (ca. 1265), stated as a certainty that “after Antichrist’s death the Lord will not come immediately to judgment” but, rather, that there would be a time of “refreshment of the saints” when “the Jews will be converted to the faith and the Holy Church will peacefully conquer everything up to the ends of the earth.” Like several other exponents of post-Antichrist chiliasm, Hugh Ripelin was probably not influenced by Joachim of Fiore. Undoubtedly, many others were. But as long as they affirmed, as almost all did, that the dispensation of the last age would be a renewal and amplification of the Christian dispensation announced in the Gospels rather than a supercession of it, they were not stating anything controversial.

With theologians agreeing that there would be good times on earth after Antichrist, anyone in the mid-fourteenth century who assumed that Antichrist's open reign would be preceded by heralding disasters could easily incorporate the Black Death into a chiliastic script. Such a script, in fact, exists in John of Rupescissa's still unpublished *Liber secretorum eventuum*, completed in November 1349.¹⁵ Rupescissa, a Franciscan visionary who was then being held in a papal prison in Avignon because of his attacks on the Franciscan and ecclesiastical hierarchies, predicted that Antichrist would triumph and reign openly for three and a half years before 1370 and that, before then, numerous chastisements would precede his arrival. First among these was the famine and plague Rupescissa



Figure 4: The triumph of the "holy people of God" on earth after the death of Antichrist. A conception descending from Rupescissa in Telesphorus, *Libellus* . . . , written toward the end of the fourteenth century. The illumination is reproduced from Munich, Bayerische Staatsbibliothek, MS Clm 313, f. 38v, executed shortly after 1431 in or around Salzburg.

cissa dated to 1347, and second was the piling up of cadavers of the year 1348.¹⁶ Famine and plague would be followed by earthquakes and other disasters, culminating in the reign of Antichrist. But in 1370 Christ would slay Antichrist and ordain a literal millennium—one thousand years of earthly blessedness before the End. During the forty-five years between 1370 and 1415, there would

¹⁵ For the only reliable original survey of Rupescissa's life and prophetic thought, see Jeanne Bignami-Odier, *Études sur Jean de Roquetaillade (Johannes de Rupescissa)* (Paris, 1952); and, for the *Liber secretorum eventuum* specifically, see *ibid.*, 113–29. Marjorie Reeves has provided an English summary of Bignami-Odier's work; *Influence of Prophecy in the Later Middle Ages*, 225–28, 321–24. Lynn Thorndike has edited the chapter headings of the *Liber secretorum eventuum* as they appear in BN, MS lat. 3598; see Thorndike, *A History of Magic and Experimental Science*, 3 (New York, 1934): 722–25. For a description of this manuscript, see Bignami-Odier, *Études sur Jean de Roquetaillade*, 239–40.

¹⁶ BN, MS lat. 3598, f. 4r–v: "Quarto intellexi multas clades futuras erunt in ianuis et evenire validas tempestates. Primo famem generalem simul cum mortalitate in orbe que facta est anno Domini M^occc^oxlvi^o per seculum universum. Secundo multiplicacionem innumerabilium cadaverorum anno Christi M^occc^oxlvi^o ubique terrarum multifarie [MS: "multifamine"] dispersorum."

still be wars resulting in the transference of the Roman Empire to Jerusalem, but then would come a millennium of the greatest possible earthly perfection when men would beat their swords into plowshares and live under the fullness of the Holy Spirit in unprecedented peace and justice. Only toward the end of the millennium would charity begin to grow cold until Gog and Magog would arrive around 2370, presaging the Last Judgment and the end of the world.

Rupescissa's view of the future was obviously shaped by knowledge of post-Antichrist chiliasm in its Joachite cast, but autobiographical passages in his writings display how he was moved to his own formulation of it by personal stress. In December 1344, he had been arrested without any warning by order of the Franciscan Provincial of Aquitaine and dragged from his native convent of Aurillac to imprisonment in the convent of Figeac, where he was held in particularly noisome quarters. In February 1345, he broke his leg and was forced two or three times onto a device to set it, which made him suffer, according to his account, the torments of the martyrs. Then he was left lying on a cot virtually without attendance for three and a half months while so many maggots crawled in his festering wound that they could be gathered by the handful. By the summer Rupescissa had recovered, but he still was in chains, surrounded by stench, and, hence, spent his days in tears, vigils, and prayers, seeking to understand why he had been made to suffer so terribly. Then, all of a sudden, in late July (one can imagine the heat), while he was standing in prayer, he was granted a miraculous flash of insight into the entire course of the present and future. This revelation made it clear that Antichrist would soon triumph and that Rupescissa was suffering as a "witness" against Antichristian forces but that there would soon be millennial rewards for the witnesses and martyrs after Antichrist's destruction.¹⁷

Rupescissa's revelation of July 1345 could not have included foreknowledge of the Black Death, but he soon learned of that horror very personally. In 1348 he became infected with the plague himself while he was imprisoned in another miserable Franciscan confinement, this time at Rieux, south of Toulouse.¹⁸ Rupescissa escaped with his life but again suffered greatly and saw, whether in reality or delirium, swarms of terrible flies that seemed like apocalyptic locusts.¹⁹ Thus, not surprisingly, he built the plague into his understanding of present and future between 1348 and 1349. God was punishing humanity in numerous ways that would culminate in Antichrist's imminent triumph, but proper understanding of the future offered a beacon of hope and a guide for enduring through tribulation.

¹⁷ For Rupescissa's arrest, imprisonment in Figeac, and revelation of July 1345, see Bignami-Odier, *Études sur Jean de Roquetaillade*, 17–18 (drawing upon Rupescissa's *Liber Ostensor* of 1356), 114 (translating from Rupescissa's *Liber secretorum eventuum*). To my knowledge, Rupescissa was the first medieval writer to espouse literal chiliasm or millennialism; I intend to write further on this and other noteworthy themes in Rupescissa's prophecies.

¹⁸ Bignami-Odier, *Études sur Jean de Roquetaillade*, 20.

¹⁹ Rupescissa, Commentary on the *Oraculum Cyrilli*, written over several years between 1345 and 1349, BN, MS lat. 2599, f. 167v: "Nota quod a. D. Mcccxlviij, qui est annus pestis magne ire Dei, vidi coram me quoddam genus muscarum. . . ."



Figure 5: The Holy Spirit raining down on the elect after the death of Antichrist, with a "holy pope" below, from Telesphorus, *Libellus*. . . . This copy was made in 1469 in the Venetian monastery of St. Cyprian; Venice, Biblioteca Marciana, MS Lat. Cl. III, 177 (2176), f. 35r. (Photograph provided by Professor Robert E. Kaske, Cornell University.)

Although Rupescissa's *Liber secretorum eventuum* was meant only for the private reference of Cardinal Guillaume Court, who was then investigating the imprisoned prophet's orthodoxy, the work traveled quickly beyond Avignon and had a very wide circulation. Five surviving Latin copies and one complete Catalan translation are only partial testimonies to its popularity, for the *Liber* became known very rapidly to chroniclers and was frequently excerpted or abbreviated.²⁰ Quite clearly, it struck upon some responsive chords.

²⁰ In addition to the three Latin copies and Catalan translation listed by Bignami-Odier (238–42), there are also two hitherto unnoticed Latin copies: Turin, Biblioteca Nazionale Universitaria, MS K² IV 13, ff. 137r–67v (mid-fifteenth century); and Milan, Biblioteca Trivulziana, MS 199, ff. 21v–29r (year 1496). The German chronicler Conrad of Halberstadt presented a resumé of the *Liber secretorum eventuum* sometime before 1353. The *Liber secretorum eventuum* was also known to the author of the *Chronographia regum Francorum* and to Jean le Bel. Bignami-Odier, *Études sur Jean de Roquetaillade*, 113, 127 n. 3, 219, 221, 233–34. A complete manuscript abbreviation is in Rouen MS 1355, ff. 90r–91r (ca 1400); and, for extracts, in addition to those in the manuscripts listed by Bignami-Odier, see Basel MS F v 6, f. 130v (ca. 1420) and Tours MS 520, ff. 47v–48r (year 1422). A fifteenth-century marginalist in the Tours manuscript refers to having another complete parchment

AS OPPOSED TO POST-ANTICHRIST CHILIASM, the pre-Antichrist variety had virtually no biblical underpinning and, therefore, was seldom espoused openly by theologians. Nonetheless, it appears to have been more "popular" than the post-Antichrist form, in the sense both of having been expressed more frequently and of having had wider currency among nonliterate classes. Ever since the eleventh century, the view that a last great emperor would inaugurate wondrous times on earth before the appearance of Antichrist had enjoyed great favor in Western Europe as a result of its circulation first in the extremely popular prophecies of Pseudo-Methodius and the "Tiburtine Sibyl" and then in numerous adaptations and imitations that often introduced kings or popes in place of the original emperor. Such prophecies were naturally reformulated and recirculated by dynastic and papal propagandists, and they unquestionably found resonance among the masses who longed for right order to be installed by epic heroes. Pre-Antichrist chiliastic prophecies were very often pseudonymous or anonymous, but that by no means impeded their circulation.²¹ Indeed, considerable evidence, most of which has hitherto been unstudied, shows that different contemporaries independently fitted the Black Death into the pre-Antichrist chiliastic scheme.

In their discussions of the background for alleged flagellant eschatological radicalism, Haupt, Cohn, and Erbstösser introduced two examples of the second variety of medieval chiasm, but, for different reasons, these are the least conclusive.²² One is the oft-cited report by John of Winterthur that around 1348 people "of all sorts" in Germany said that the emperor Frederick II would return to persecute the clergy, reform the Church, redistribute wealth, and reign in justice before resigning his crown on the Mount of Olives. As feverishly intense as these expressions may have been, they cannot be associated directly with the incidence of the Black Death because, as Haupt and others have hesitated to acknowledge, they were current in Germany before the plague struck.²³

The second example was connected with the Black Death, but, unfortunately, it survives in a form that is barely usable. According to the Würzburg chronicler Michael de Leone, a certain "great astrologer" "predicted" for 1348 the coming of a "great dearth and pestilence." No doubt this was prediction after the fact; Michael himself clearly recognized the pestilence as the Black Death, because in 1349 he commented that such was "already coming true in many parts of Lom-

copy. (I used a microfilm of Tours MS 520 generously lent to me by the Institut de Recherche et d'Histoire des Textes, Paris.)

²¹ For an excellent survey of such prophecies that circulated before the fourteenth century, see Töpfer, *Reich des Friedens*, esp. chaps. 1, 4.

²² Erbstösser's finding of chiliastic expectations in verses added to the *Chronica S. Petri Erfordensis* is untenable: these do not refer to a "returned Frederick" but merely to the succession of Margrave Friedrich III of Meissen on the death of his father, Margrave Friedrich II; for Erbstösser's remark, see *Sozialreligiöse Strömungen im späten Mittelalter*, 32.

²³ Vitoduranus, *Chronica*, 280–81. For an English translation, see McGinn, *Visions of the End*, 251; for the best of the numerous treatments of this passage, see Töpfer, *Reich des Friedens*, 178–82. John's chronicle stops in 1348, before the Black Death had spread to Germany.

bardy.”²⁴ The astrologer also “predicted” many other events, but there are doubts about whether Michael de Leone reported them in correct order or whether he abbreviated them excessively. For example, the list of predictions contains a brief statement that the “tyrannous king of France would fall with all of his barons”; this sounds very much like a prophecy after the fact of Philip VI’s defeat at Crécy in 1346, but that would make it two years off the mark and, coming after the Black Death, not in proper order. Thus, it is difficult to know how to order the astrologer’s predictions of disasters that had apparently not yet come, such as an infestation of insects and poisonous animals, or exactly how to interpret his predictions of the coming of “a single lord” and “the exaltation of the Roman Empire.” As best as can be told, however, the last two predictions appear to have been expressions of the perennial imperial prophecies that foretold the coming of a great messianic ruler before the time of Antichrist.²⁵

Both John of Winterthur and Michael de Leone were chroniclers who told of prophecies at second hand. Scholarly reliance on them has been typical of a general reliance on chronicle sources as evidence for medieval eschatological ideas. But numerous prophecies that circulated independently of the chronicles help bring us much closer to the original nature and quality of medieval popular eschatology. Just as John of Rupescissa’s unpublished *Liber secretorum eventuum* shows how the Black Death was fitted into the post-Antichrist pattern of chiliasm, so several other unnoticed prophecies reveal how the epidemic was fitted into the pre-Antichrist pattern.

The most detailed example is the neglected prophecy of an obscure Frenchman, John of Bassigny.²⁶ All we know about this prophet, other than that Bas-

²⁴ Michael de Leone, *Annotata historica* [more properly, *De cronicis temporum modernorum hominum*], ed. J. F. Böhrmer, *Fontes rerum Germanicarum*, 1 (1843; reprint ed., Aalen, 1969): 474. For the dating of Michael’s history to June 1349, see Stuart Jenks, “The Black Death and Würzburg: Michael de Leone’s Reaction in Context” (Ph.D. dissertation, Yale University, 1976), 37.

²⁵ Stuart Jenks has observantly noticed that the “great astrologer’s” prediction “unus solus erit dominus” echoes Ezekiel 37: 22—“rex unus erit dominus”—which, according to Vitoduranus, *Chronica*, 281, was a prediction cited to express hopes for the return of Frederick II around 1348; Jenks, “Die Prophezeiung von Ps.-Hildegard von Bingen: Eine vernachlässigte Quelle über die Geisslerzüge von 1348/49,” *Mainfränkisches Jahrbuch*, 29 (1977): 9–38, at 35 n. 51.

²⁶ The only prior published treatments, excluding the fanciful nineteenth-century ones (see note 33, page 547, below), are Noël Valois, “Conseils et prédictions adressés à Charles VII, en 1445, par un certain Jean du Bois,” *Annuaire-Bulletin de la Société de l’Histoire de France*, 46 (1909): 201–38, at 223–25; and Thorndike, *A History of Magic and Experimental Science*, 312–15. Valois referred to John only insofar as he is used by the fifteenth-century Jean du Bois; Thorndike treated John’s prophecy more extensively but without knowledge of the prophetic tradition within which John worked and without sufficient knowledge of the extant manuscripts. Valois and Thorndike knew two copies of John of Bassigny’s prophecy: BN, MS lat. 7352, ff. 2r–4v (fifteenth century); and Tours MS 520, ff. 146r–49v (year 1422). I have relied on Valois’s and Thorndike’s works for readings from the Bibliothèque Nationale manuscript. I have found four more copies that are independent of the late *Mirabilis Liber* printed version (note 33, page 547, below): Kues, Hospitalbibliothek, MS 57, ff. 103vb–04rb (first half of the fifteenth century) (Sara Clark kindly lent me a microfilm of this copy); Bamberg, Staatsbibliothek, MS Msc. Astr. 4, ff. 155r–57v (first half of the fifteenth century) (f. 157 is bound out of order; it should precede f. 155); BL, MS Cotton, Cleopatra C IV, ff. 81v–85v (late fifteenth century); and BL, MS Lansdowne 762, ff. 54v–57v (early sixteenth century). The last two copies are too corrupt to be of help in establishing readings, but the Kues and Bamberg texts are important witnesses. Unfortunately, the Kues copy breaks off less than halfway through the text; I follow it where I can and use the Bamberg copy for the rest. A complete study and critical edition of the Bassigny prophecy would be a valuable contribution to prophetic and mid-fourteenth-century French history.

signy was a small territory in Champagne, comes from the text of his prophecy—and that may not be too trustworthy. John claimed to have derived his knowledge of future events not just from studying Holy Scripture and the writings of the prophets, poets, and many learned authorities but also from conversations he held with a Syrian and a “Chaldean” in 1341 while he was traveling in the Holy Land. But this account may not be reliable, because the Eastern place names he gave appear to be bogus.²⁷ John also claimed to be predicting events from 1343 onward, but his “prophecies” for the years up to about 1361 were clearly made from a knowledge of events that had already occurred.²⁸

According to John of Bassigny, a terrible time of trials for the world would begin in 1343 or 1344 with the onset of a devastating plague.²⁹ This was certainly the Black Death, because John described it tellingly as a “general mortality and pest” that would carry off more than half of the population.³⁰ Most likely he dated its onset three or four years earlier than the time it first appeared in the West because he was attributing his “prophetic” information to Easterners. John must have assumed that the plague moved from east to west and had no way of knowing when it might have begun in the East. But to leave no doubt in his readers’ minds that he was alluding to the Great Plague, he went on to say that it would last for “45 months or more,” thereby specifying the actual time of the Black Death’s visitation.

In the wake of the plague would follow a succession of horrendous chastisements. Among other things, in 1346, “or a little earlier or later” (a studied vagueness), a great “prince and king of all of the West” would be miserably chased and defeated in battle, and almost all of his knights would be killed—surely a prophecy after the event of the French defeat at Crécy. Similarly, John’s “prediction” that “around 1356” the king of France would be captured by his enemies was a prophecy after the event of John the Good’s capture by the English at Poitiers. John “predicted” still worse disasters for France for 1358 and 1359, the actual period of the *Jacquerie*, the revolt of Étienne Marcel, and the

²⁷ In the Bibliothèque Nationale manuscript, John claimed to have spoken with a Syrian in “Gadis subtus Quadrum,” with a Chaldean in “Bethsedin iuxta Montem Thabor,” and with a Jew in “garda Ademari”; BN, MS lat. 7352, as given by Valois, “Conseils et prédictions adressés à Charles VII,” 223 n. 1. So far as I can see, the Kues, Bamberg, and Tours manuscripts all omit reference to the Jew and give, respectively, the following forms for the first two place names: “Gradris subtus Quadum,” “Betseladim iuxta Montem Tabor”; “Gadzis subtus Quadrum,” “Bethseladum iuxta Montem Thabor”; and “Gadris subtus Cadrum,” “Seboch iuxta Montem Thabor.” Of all of these names, the only one that appears in Graesse-Benedict’s *Orbis Latinus* is Bethsedin—the biblical Bethsaida—which is near, but not immediately next to, Mount Tabor. John may have chosen to mention Bethsaida because Antichrist was expected to be nurtured there; see, for example, Adso Dervensis, *De ortu et tempore Antichristi*, ed. D. Verhelst (Turnhout, 1976), 24.

²⁸ Compare Valois, “Conseils et prédictions adressés à Charles VII,” 223 n. 1: “... prédictions rédigées entre les années 1342 et 1345.” Thorndike, *A History of Magic and Experimental Science*, 314, is properly more skeptical.

²⁹ Kues, Hospitalbibliothek, MS 57, f. 104ra: “Nam idem ille annus Domini Mcccxlili erit initium omnium dolorum, quoniam in ipso anno incipiet et eveniet quedam generalis mortalitas et pestis que universum mundum mutabiliter vexabit et affliget, ita quod bene fere plusquam media pars, velut verius dicere due partes, hominum mundi morientur. Et infra quadragesimum quintum mensem, que pestis pro certo xlv menses durabit, vel amplius, licet regnet et vadat modo ibi, modo alibi.” The Bamberg manuscript and a marginal note in the Tours manuscript give 1344 for the beginning of the plague; other manuscripts give different dates, but 1343 and 1344 are the only ones that fit with the chronology of the rest of the prophecy.

³⁰ The term “Black Death” was not a contemporary one; chroniclers usually used the terms “grandis mortalitas” or “grandis pestis”; see Dols, *The Black Death in the Middle East*, 5 n. 5.

ravaging of France by the English. Starting around 1361 or 1362 John introduced real prophecy, predicting, for example, the destruction of Paris before 1362.³¹ After that, the Church would be terribly persecuted, and the elements would rage.

So far, all was bad, but John was no unmitigated pessimist. A coming young hero (perhaps meaning the future Charles V) would assume the French crown and dominate the world. He would aid the Irish and Scots in invading England and in annihilating the "sons of Brutus" so that there would thereafter be no memory of them. A holy pope would be crowned by angels and bring the Church back to its pristine state of apostolic sanctity. He would go preaching barefoot everywhere and convert the infidels. Then he would ordain a messianic ruler from the "most noble" French line as emperor, and the two would reform and bring peace to the whole world. Under their sway, there would be "one law, one life, and one faith," and all men would be of one spirit and love each other. This time of peace would last for "many years," but then men would return to evil, and times would grow worse until the coming of Antichrist.³²

John of Bassigny's prophecy circulated quickly—one copy reached northern England a few years after it was written³³—and must have satisfied a deep need

³¹ The Bibliothèque Nationale copy that Thorndike used has 1382, but I follow the Bamberg manuscript, which consistently presents the most coherent dates.

³² Bamberg, Staatsbibliothek, MS Msc. Astr. 4, f. 156r: "Ordinabit autem Deus secum unum imperatorem sanctissimum qui erit de reliquis et nobilissimi sanguinis et seminis Francorum regum. Et erit sibi in adiutorium et obediens in omnibus mandatis eius ad reformandum in melius universum orbem. Sub ipsis autem papa et imperatore pacificabitur omnis orbis, quoniam ira Domini quiescet; et sic erit una lex, una vita, et una fides, et erunt homines unanimes, et invicem se amantes, et concordantes. Durabitque pax per annos multos. Postquam autem seculo in melius reformato, iterum multa signa in celo apparebunt qua malicia hominum se evigilabit, et ad mala pristina et malicias pessimas homines revertentur . . . et tunc apparebit Antichristus."

³³ P. Meyvaert, "John Erghome and the *Vaticinium Roberti Bridlington*," *Speculum*, 41 (1966): 656–64, at 658; and Reeves, *Influence of Prophecy in the Later Middle Ages*, 254–56. A bizarre resurrection of the Bassigny prophecy in revolutionary and postrevolutionary France is worth a full study but may be alluded to briefly here. The story begins with the publication of a very corrupt version of the text, ascribed to "Johannes de Vatuiguerro," in the *Mirabilis Liber*, a patriotic French prophetic anthology published at least six times in Paris in the 1520s and once in Rome in 1524. (Occasional reference to a Venetian edition of 1514 is clearly a bibliographical error. Dated Parisian editions appeared in 1522, 1523, and 1524, and at least three others were published around the same time in Paris without date "au Pellican," "au Roi David," and "à l'Éléphant." In the undated "au Pellican" edition, the "Vatiguerrus" prophecy appears at folios 55r–58r; in the Rome edition, it is at folios 46r–48v.) Centuries later, in 1795, a French Royalist who read the "Vatiguerrus" prophecy in a copy of the *Mirabilis Liber* he found in Paris in the Bibliothèque Nationale concluded that it applied perfectly to the present tribulations of France and also foretold a Bourbon restoration. The results were that many others flooded the Bibliothèque Nationale to read the notorious prediction, that the police cracked down and imprisoned a librarian for making the *Mirabilis Liber* available, and that copies of the "Vatiguerrus" prophecy circulated clandestinely throughout France in a tendentiously altered version fathered on St. Cesarius of Arles. After the Restoration the prophecy could once more go public, and thus in 1814 and 1815, and then again after the Revolutions of 1830 and 1848, numerous editions appeared of either the "Cesarius" text or the unaltered "Vatiguerrus" one, printed in Latin or French or in both languages simultaneously. For an illustrative example, see [Hyacinthe Olivier-Vitalis, ed.] *Prophétie recueillie et transmise par Jean de Vatuiguerro; extraite du Liber Mirabilis* (Carpentras, 1814); I have used a copy in Avignon, Bibliothèque Calvet, In-8°24,897. Olivier-Vitalis explained how, as librarian at Carpentras, he had earlier hidden away three copies of the *Mirabilis Liber* to make sure that the police would not confiscate them and how he was now free to bring out the "Vatiguerrus" text publicly in Latin with facing-page French, together with a commentary showing how it accurately predicted, among other things, the death of Louis XVI, the persecution of the Church, and the return of the Bourbons, to be followed by a long reign of peace. For more, but by no means exhaustive, information on the "Vatiguerrus"-"Cesarius" copies, see Abbé Lecanu, *Dictionnaire des prophéties et des miracles*, 2 (Paris, 1852): 54–61, 716; and Jean Harmand, "Une Prophétie du XVI^e siècle sur la Révolution," *Revue des études historiques*,

for comprehending the present within the terms of the future. The one drawback to using it as a source for appraising eschatological reactions to the Black Death is that it was not written under the most immediate impact of the plague, at least not at the time of the first occurrence. But another prophetic text, almost entirely neglected by scholarship, dates from the terrible plague year of 1349 itself.

The Carmelite William of Blofield sent a report of "rumors" from "Roman parts" to an unnamed friar in the Dominican convent of Norwich in 1349. According to these rumors, Antichrist was already ten years old and a boy of incomparable beauty and learning. But another boy who lived "beyond the Tartars" and was instructed in Christianity was already twelve years old. He would destroy "the perfidy of the Saracens," become the greatest among Christians, and rule as pope and emperor. His empire, however, would quickly come to an end by violence and thereafter would arise unprecedented "revolutions" throughout the world, out of which would emerge a "good and just pope" who would create cardinals who feared the Lord. In this pope's time there would be the greatest peace, but afterward Antichrist would reign openly. William of Blofield dismissed these rumors as fictitious, but he was nonetheless sufficiently interested in them to communicate them to a Dominican friend, who in turn preserved them at Norwich. Whatever William thought, his report is evidence that speculations about Christian triumph and wondrous times before Antichrist circulated in Italy during the time of the plague.³⁴

Still more evidence shows that similar speculations were circulating very widely at the same time throughout Western Europe. In the Middle Ages prophecies did not always have to be invented to fit new situations; old ones could be resurrected with new dates. One that was resurrected to fit the Black Death was a text beginning "the high Cedar of Lebanon will be felled," which circulated in various forms throughout Europe from about 1240 until deep into the seventeenth century and lay at hand during the plague years for someone to revive for the edification of himself and others. The version of the "Cedar of Lebanon" prophecy he used began by reporting that in 1287 a Cistercian monk in Syrian Tripoli had seen a hand writing a prophetic message during mass on the corporal cloth over the altar. The message foretold that Tripoli and Acre

79 (1913): 523-49. Neither Lecanu nor Harmand knew that the "Vatiguerrus" prophecy was originally written in the fourteenth century by John of Bassigny.

³⁴ Cambridge, Corpus Christi College, MS 404, f. 102; I edit this text in the appendix, page 552, below. Marjorie Reeves edited without comment the first half of this text; *Influence of Prophecy in the Later Middle Ages*, 94. On William, see A. B. Emden, *A Biographical Register of the University of Cambridge to 1500* (Cambridge, 1963), 66. Emden has shown that William belonged to the Carmelite convent of Cambridge in 1343. Although the Cambridge text presents some ambiguity, I prefer to think that William sent his report to Norwich from Italy rather than from some place in England. (It is noteworthy that Blofield is only about ten kilometers away from Norwich; the recipient of William's report may have been a relative or local friend.) On the copyist of the Cambridge manuscript, Henry of Kirkstede, who was the librarian of Bury St. Edmunds, see Richard H. Rouse, "Bostonus Buriensis and the Author of the *Catalogus Scriptorum Ecclesiae*," *Speculum*, 41 (1966): 471-99. Henry found William of Blofield's report in Norwich on his bibliographical travels and expressed his own doubts about the rumors by writing in the margin of his copy *mendacium*. My study of the manuscript in connection with the "Cedar of Lebanon" prophecy leads me to believe that Henry's copy dates from between ca. 1365 and ca. 1377; for the "Cedar of Lebanon" prophecy, see note 35, page 549, below.

would soon fall and that worse disasters would follow: a "people without a head" would come, the "Ship of Peter" would be tossed in the waves, and battles, famines, and plagues would strike everywhere. Then two great rulers, one from the East and the other from the West, would conquer the world and bring peace and "abundance of fruit" for fifteen years. Thereafter would follow a successful crusade, the city of Jerusalem would be "glorified," and the Holy Sepulchre would be visited by all. But in this tranquillity "news would be heard of Antichrist."³⁵

The vision of the Cistercian monk was certainly fictitious; much of it was plagiarized from a prophecy of about 1240, and there was not even any Cistercian cloister in Syrian Tripoli.³⁶ Nonetheless, it took Europe by storm in the last years of the thirteenth century, no doubt because it so accurately "foretold" the fall of Tripoli and Acre (in fact, more prophecy after the event) and placed those events within a larger prophetic context. Around the time of the Black Death it could be found in numerous libraries and obviously appeared to someone to contain accurate predictions of current events—namely, the coming of "plagues in many places," and the coming of a "people without a head," who could be seen as the flagellants ("without a head" because they had no known leader).³⁷ Since the vision "predicted" the present so well, the unknown reader must have decided that here was a certain guide to the future and therefore decided to circulate it for the benefit of his contemporaries. Instead of recirculating it untouched, however, he rewrote the introduction to state that the Cistercian monk saw his vision in Tripoli in 1347.³⁸ Surely he did this to make his vision seem more immediate, but he clearly did not give his alteration much careful thought: in 1347 the Holy Land had been lost for more than a half-century, and it was ridiculous to imagine a Cistercian cloister there or a message predicting the fall of cities that had long since fallen.

Still, the version of the "Cedar of Lebanon" prophecy for 1347, which otherwise contained only minor revisions, was an enormous success. There are at least eight surviving manuscript copies, and the textual variants that appear in these copies are so multifarious that there could easily have been more than one hundred copies now lost.³⁹ Short prophecies sometimes circulated on single sheets

³⁵ For published editions of versions close to the one used by the mid-fourteenth-century reviser, see *MGH, Scriptores rerum Germanicarum*, 17: 605, and 23: 567–68. I am currently working on an extended study of the "Cedar of Lebanon" prophecy, which will provide editions and more information concerning the various versions.

³⁶ See my "Medieval Prophecy and Religious Dissent," *Past & Present*, no. 72 (1976): 3–24, at 12.

³⁷ For an explicit statement that the "people without a head" in the prophecy were the flagellants, see Breslau (Wrocław), University Library, MS IV F 6, f. 100v; for a published edition, see Joseph Klapper, ed., *Exempla aus Handschriften des Mittelalters* (Heidelberg, 1911), 64 (which was kindly called to my attention by Dr. P. Dinzelsbacher, Stuttgart). For independent designations of the flagellants as "gens sine capite," also see Erb-stösser, *Sozialreligiöse Strömungen im späten Mittelalter*, 27; and Jenks, "Die Prophezeiung von Ps.-Hildegard von Bingen," 20.

³⁸ For the only published edition of this version, see Jean Leclercq, "Textes et manuscrits cisterciens dans des bibliothèques des États-Unis," *Traditio*, 17 (1961): 163–83, at 166–69. The manuscript that Leclercq used gives the date 1346 for the vision, but I believe this to be a variant from the original revision's 1347. Leclercq's commentary is based on the fallacious assumption that the single copy he used was unique.

³⁹ The eight medieval copies I know are Oxford, Bodleian Library, MS Bodley 761, f. 184v (Frau Wal-

that were not subsequently preserved.) The eight copies also bear witness to a very wide geographical distribution, having been copied in places as distant from each other as Yorkshire, Catalonia, the Rhineland, and Lower Austria.⁴⁰ Some of the copies were made after the plague subsided, but some circulated while the epidemic was raging: the copy from Lilienfeld, Lower Austria, concludes with a report of the desolation wrought by the plague in Avignon. Like John of Bassigny's prophecy and the rumors reported by William of Blofield, the revised version of the "Cedar of Lebanon" vision must have been conceived in order to relate the woes of the present to the certainties of the future. Despite its chronological absurdities, it circulated widely because it helped many to fathom the otherwise unfathomable.⁴¹

THE WIDE CIRCULATION of the "Cedar of Lebanon" vision makes it abundantly clear that chiliastic prophecies were not exclusively spread by flagellants, or (if at all) by "fanatics" or heretics who tried to play on the despondency of the dislocated lower classes. None of the copies was accompanied by any call for violent action, and all whose provenance can be determined came from monastic or aristocratic milieux. Similarly, John of Bassigny's prophecy was transmitted and studied by clerics,⁴² and William of Blofield said nothing about the rumors he reported having been spread by flagellants, rabble-rousers, or heretics. If we include the prophecy of Michael de Leone's "great astrologer" in the list of chil-

traud Huber kindly called my attention to this copy); Oxford, Bodleian Library, MS Digby 218, f. 107r; Oxford, Bodleian Library, MS Fairfax 27, f. 26r; BN, MS Français 902, f. 96v; Yale University Library, MS Marston 225, ff. 43v-44v (the copy that Leclercq edited); Munich, Bayerische Staatsbibliothek, MS Clm 28229, f. 21r; MS Lilienfeld 49, f. 357r (a copy I learned of by means of the incipit list of the Hill Monastic Manuscript Library, St. John's University, Collegeville, Minn.); and Carpentras MS 336, ff. 75v-76v (a Catalan translation). A seventeenth-century copy of the Fairfax 27 text appears in Oxford, Bodleian Library, MS Fairfax 28, f. iii^v.

⁴⁰ The Bodleian Fairfax 27 copy is from Bolton, Yorkshire; the Carpentras copy is from Catalonia; the Munich copy is from Speyer on the Rhine (I am grateful to Dr. Alexander Patschovsky for this identification); and the Lilienfeld copy is from Lilienfeld, Lower Austria.

⁴¹ Another prophecy that may have been redated to relate to the Black Death derives from the *De semine scripturarum*. In the original version of this early thirteenth-century work, which is still unedited, the time of the letter "x" (in which the Church was to be cleansed of corruption) was to last from 1215 to 1315, and the time of the letter "y" (one of the conversion of all peoples) from 1315 to 1415. See Töpfer, *Reich des Friedens*, 46; and H. Grundmann, "Ueber die Schriften des Alexander von Roes," *Deutsches Archiv für Erforschung des Mittelalters*, 8 (1950): 162. But in a Würzburg University Library manuscript (M p. mi. f.6, f. 37r), the time of "x" is altered to the years 1248 to 1348, and of "y" to the years 1348 to 1448; see H. Grauert, *Magister Heinrich der Poet in Würzburg und die römische Kurie*, Abhandlungen der königlichen Bayerischen Akademie der Wissenschaften, philos.-philol. und hist. Klasse, 27, 1-2 (Munich, 1912), 443-44. Also see Jenks, "Die Prophezeiung von Ps.-Hildegard von Bingen," 17, 35 n. 54. Jenks has read the date given for the prophecy of Merlin in the manuscript following the references to *De semine scripturarum* as 1343; but to my mind this dating does not necessarily conflict with Grauert's view that the text was copied around 1350. Since the manuscript was written under the direction of Michael de Leone, it is possible that the redating was done by Michael or someone shortly before him as a way of reconceiving the prophecy in terms of a newly perceived significance found in the plague. Further study of the textual traditions of *De semine scripturarum* might help confirm or disprove this hypothesis.

⁴² Both the Augustinian friar John Erghome and the parish priest Guillaume Bauge of Nouans in the diocese of Tours, whose copy was used for the edition in the *Mirabilis Liber*, studied John of Bassigny's prophecy. The copy of Bassigny's prophecy in the Tours manuscript collection was made in the Benedictine monastery of Marmoutier. On John Erghome, see Meyvaert, "John Erghome and the *Vaticinium Roberti Bridlington*," 656-64; and Reeves, *Influence of Prophecy in the Later Middle Ages*, 254-56.

iastic texts inspired by the plague, we have still another prophecy that traveled in thoroughly respectable and orthodox circles.

All of this should not be surprising since it was obviously not necessary to be aberrant and poor to be upset by the plague, and resort to prophecy was meant to provide edification and comfort, not inspiration for insurrection. Europeans tried to comprehend the fury of the plague with the aid of what might be called a prophetic "deep structure." The prophecies I have introduced were certainly conceived independently, but, aside from variations in details, all foresaw contemporary storms being succeeded by wondrous times of peace and Christian triumph. One might make an analogy to the calm after the storm in Beethoven's *Pastoral* Symphony, were it not for the fact that the prophetic calm after the storm was not the finale but was expected to be followed by either the reign of Antichrist or the advent of Gog and Magog, with the Last Judgment to follow in either case thereafter. Peace and tranquillity on earth might be great but earthly attainments could never be perfect or eternal, for Christianity was based on the assumption that perfection could only be found in the hereafter and the beyond.

The reasons for the similarities in the plague prophecies were probably twofold. First, as we have seen, fully developed prophetic traditions lay behind all of the texts. Despite his assertion of sudden illumination, Rupescissa's predictions rested on a long tradition of post-Antichrist eschatological exegesis, and prophecies of coming messianic rulers and times of peace before Antichrist had circulated in Christian Europe before 1347. Within certain limits prophets were free to alter details: they might state that the coming new age would be longer or shorter or that it would have more of one kind of progressive fulfillment than another. But within both the post- and pre-Antichrist alternatives there were basic sequences of events that prophets were virtually obliged to follow.

And, second, just those basic sequences were designed to provide comfort. Present disasters might be tolerated better if they could be viewed in terms of a coherent divine plan. Chastisements might come, but it was surely comforting to know that they would have an end and be followed by "peace and tranquillity." So whenever new disasters struck—the Black Death is by no means the only example⁴³—new prophecies were brought forth out of the "deep structure" or old prophecies were retailored to fit new events.

Mentalities, like Mediterranean sailing routes, have their *longues durées*. The Black Death, medieval Europe's greatest disaster, prompted many to think about how the present related to the future and called forth expressions of chiliasm that circulated from Italy to England and from Austria to Catalonia. In their main outlines, these expressions were not new but were manifestations

⁴³ For three prophecies in addition to the "Cedar of Lebanon" text that seem to have been inspired by the fall of Tripoli and Acre, see my "Medieval Prophecy and Religious Dissent," 14. Numerous examples of other prophecies inspired by other disasters or portents could be adduced. The Black Death itself seems to have inspired fewer prophecies than one might have expected, but as yet this is only an impression; perhaps other prophecies circulated that are still unidentified or lost, and perhaps some older prophecies were applied by contemporaries to the Black Death in ways that are still unknown. A comparison between the number and nature of prophecies inspired by the Black Death and other events that were perceived to be disastrous might be instructive; but at present that project seems too difficult to undertake.

of a basically unchanging medieval prophetic structure. They were meant to inspire perseverance in faith, hope, and penance, but they were not otherwise meant as calls to action. They intended to give comfort by providing certainties in the face of uncertainty and must have helped frightened Europeans get about their work.⁴⁴ In such ways can mentalities, like sailing routes, support life.

⁴⁴ Compare M. Dols, "Comparative Communal Responses to the Black Death in Muslim and Christian Societies," *Viator*, 5 (1974): 269-87; and his *Black Death in the Middle East*, 281-302. Dols has emphasized the contemporary Islamic lack of eschatological hopes and consequent resignation and fatalism. Despite Dols's thoughtful work, comparisons between Western Christian and Islamic reactions to the plague must remain highly speculative, owing to the deficiencies of the sources and the complexities of the problem.

APPENDIX

William of Blofield's Report of Rumors from Roman Parts

I edit the following from Cambridge, Corpus Christi College, MS 404, f. 102r-v. For a description of the manuscript, see M. R. James, *A Descriptive Catalogue of the Manuscripts in the Library of Corpus Christi College, Cambridge*, 2 (Cambridge, 1912): 269-77; and, for further information on the manuscript, see Rouse, "Bostonus Buriensis and the Author of the *Catalogus Scriptorum Ecclesiae*," 475, *passim*; and Reeves, *Influence of Prophecy in the Later Middle Ages*, 539. Reeves edited the first half of the text; *ibid.*, 94. I adhere to the spelling of the manuscript, except in providing "v" for consonantal "u," but I have modernized the punctuation and capitalization.

Subscriptos rumores scripsit frater Willelmus de Blofeld in Anglia, anno Domini m.ccc.xlix, cuidam fratri conventus Fratrum Predicatorum Norwicensis, quod prophete diversi sunt in partibus Romanorum, sed adhuc occulti, qui omnia fictiva predixerunt per annos multos. Et isto anno, videlicet ab incarnatione m.ccc.xlix, predicant Antichristum habere se x annos etatis et puerum esse dilectissimum et doctissimum in omni sciencia, in tantum quod non est aliquis iam vivens qui sibi poterit coequari. Predicant eciam alium puerum ultra Tartaros iam natum xii annorum, qui in fide Christiana est imbutus, et hic est qui perfidiam Saracenorum destruet et maximus est inter Christianos, sed cito finietur eius imperium in adventu Antichristi. Isti eciam prophete de isto papa inter cetera dicunt quod finem violencium faciet. Dicunt eciam quod post mortem istius pape tot revoluciones erunt in mundo quot nunquam fuerunt per aliquod tempus. Sed post hec surget alius papa bonus et iustus, et cardinales creabit dominum timentes, et tempore huius maxime erit pax. Et post eum nullus erit papa, sed Antichristus veniet et se ostendet, etc.

The Social History of the American Soldier: A Review and Prospectus for Research

RICHARD H. KOHN

OVER THE COURSE OF AMERICAN HISTORY, few experiences have been more widely shared than military service. From the seventeenth century to the present, Americans by the millions have served in armed forces of one kind or another, in war and in peace, on frontiers and overseas, as career professionals and temporary militia conscripts. Universal military obligation is one of our oldest and most enduring traditions. Every generation has experienced military conflict of some kind, and through the twentieth century the military has increased dramatically in size and in its impact on national affairs. Between 1940 and 1973 the government through selective service touched the lives of nearly every American family directly, even if a male family member did not serve. In 1980 an estimated 37 percent of the male population over age seventeen were veterans, as many as 70 percent for those in the age bracket forty-five to sixty-four; and, in gross numbers, including women, some thirty million Americans were veterans in 1980. Nor are these figures likely to change significantly. Projections for manning the all volunteer army stress the need for fully one quarter of our eighteen-year-old males to enlist to maintain the armed forces at a strength of two million. And current projections estimate that 28 percent of the male population over seventeen, perhaps twenty-eight million men, will be veterans in the year 2000, even without conscription.¹

With rare exception, American historians, particularly social historians, have

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¹ Peter Karsten, *Soldiers and Society: The Effects of Military Service and War on American Life* (Westport, Conn., 1978), 3, 20; Morris Janowitz and Charles C. Moskos, Jr., "Five Years of the All-Volunteer Force: 1973-1978," *Armed Forces and Society*, 5 (1979): 180-81, 211; Richard V. L. Cooper, *Military Manpower and the All-Volunteer Force* (Santa Monica, Calif., 1977), 44, 189-93; and Harold Wool, *The Military Specialist: Skilled Manpower for the Armed Forces* (Baltimore, 1968), 104-06. In 1971 Adam Yarmolinsky estimated that "roughly one out of eight" Americans were veterans, a very high figure considering women and children in the population; Yarmolinsky, *The Military Establishment: Its Impacts on American Society* (New York, 1971), 45. For statistics on veterans, see Committee on Veterans' Affairs, U.S. Senate, *Study of Health Care for American Veterans: A Report Prepared by the National Academy of Sciences National Research Council* (Washington, 1977), 16; and U.S. Veterans Administration, *National Survey of Veterans: Summary Report* (Washington, n.d.), 6.

neglected this experience. In the last two decades scholars of the military have begun to abandon the old preoccupation with strategy and battle, but few practitioners of the "new" military history have chosen subjects that are frankly social.² Of course, there has existed for generations a vast literature on American soldiers in the form of histories, memoirs, diaries, biographies, literary studies, popular hagiographies, government compilations of statistics, and sociological studies. But little of this material has been concerned with understanding soldiers per se, the military experiences of Americans, or the impact of service on the nation. Most of the history has aimed at calling to memory the patriotism or loyalty of particular individuals and groups; the sociology, at advancing social science methodology or aiding the government in recruiting its armies and using manpower efficiently. What do we really know of this experience for Americans and, indeed, of the Americans who served through our history? Who were they? Why did they enlist or submit to coercion into service? Where did they come from? What did their leaving and their returning mean to their communities? What did they think? Why did they fight? How did they behave? What impact did service have on them and they on the nation? "Several scholarly studies of specific dimensions of the subject do exist and deserve . . . attention," one historian has written recently. "But such studies are . . . few in number" and focus primarily on the World War II and postwar eras.³ In point of fact, historians have neglected one of the most pervasive experiences in American life, one especially suited to the new social history. Because of the vast literary and statistical source material, examining service in the military ought to reveal much about the American population and society and, even further, begin to explain the significance of that service and fix it firmly in the mosaic of American history, where it has always belonged.

FOR MOST OF OUR HISTORY, our vision of the American soldier—the prototypical enlisted man in the army, navy, or militia (marines have been by their own definition atypical)—has been expressed in various forms of symbol and myth. While historians, and a good proportion of the public, might no longer accept the old stereotypes, they still persist in patriotic rhetoric, in army thinking, and even in some forms of the old scholarship.⁴ The American soldier was a cross

² In fact, there is some argument for returning to more traditional subjects. See Russell F. Weigley, "Introduction," in Weigley, ed., *New Dimensions in Military History: An Anthology* (San Francisco, 1975), 11–12; Allan Millett, "The Study of American Military History in the United States," *Military Affairs*, 41 (1977): 58–59; Gunther E. Rothenberg and Dennis Parks, "European Military History in America: The State of the Art," *ibid.*, 75–77; and John Keegan, "The Historian and Battle," *International Security*, 3 (1978–79): 140–45.

³ Karsten, *Soldiers and Society*, 3, 4.

⁴ Scholars still take sides on whether major portions of the traditional stereotype hold. The most complete general study by a scholar attempts throughout to explain away any material "derogatory to Americans in uniform." "What I really intended to do was write a tribute; simple, direct and truthful," Victor Hicken stated in his preface, chronicling the American fighting man while "sparing none of his faults, emphasizing all his virtues." Hicken, *The American Fighting Man* (New York, 1969), viii. For examples, see *ibid.*, 16, 19, 21, 32–33, 47, 139–40, 141–49, 164, 178, 185, 237. Some newer work on the American Revolutionary War soldiery also pushes older patriotic interpretations, quite unpersuasively in my opinion. See Charles Royster, *A Revolutionary People at War: The Continental Army and American Character, 1775–1783* (Chapel Hill, 1980); and Robert Mid-

section of the American population; he enlisted and fought out of patriotism; his love of liberty, intelligence, and native individualism meant that the goals of war had to be explained to him. "[M]en must be thoroughly imbued with the spirit of their cause . . . , believe in it and want it with every atom of their being," a psychological handbook counselled prospective officers in 1943, echoing a theory first publicized by Friedrich von Steuben in 1779 and repeated as dogma ever since.⁵ In battle our soldiers were courageous; if captured, they remained loyal; once the battle ended, they reverted to the kind, adaptive, happy-go-lucky scroungers of Ernie Pyle's classic World War II reporting. Even as sophisticated an observer as General S. L. A. Marshall, who as an officer and writer studied Americans in four wars and often advised the army on training and leadership, perpetuated some of these generalizations. Marshall believed our soldiers had certain common characteristics, "true in times past from Lexington . . . to Pork Chop Hill": "resourceful and imaginative . . . , to a certain extent machine-bound . . . , optimistic . . . though their griping is incessant, . . . gregarious," impatient of "spit-and-polish," possessive of "an uncanny instinct for ferreting out the truth when anything goes wrong tactically, . . . wasteful of drinking water, food, munitions, and other vital supply," and hesitant to kill. But, if "led with courage and intelligence, an American will fight as willingly and as efficiently as any fighter in world history."⁶

These myths have endured for a variety of reasons. Americans have long believed that how they have behaved in service and in battle reflected their character as a people and their virtue as a nation. Moral worth, and our special distinctiveness as a country, seemed somehow to hinge on the people's eagerness to serve, its bravery under fire, its loyalty in captivity, its virtue in bivouac, and its willingness to melt back silently into civilian life. The government down through our history has also contributed to stereotyping American soldiers. To mobilize the population and justify the cause, propaganda has consistently pictured the American in uniform in the best possible light. Veterans' organizations have also joined willingly in the celebrations of the American soldier, lest they cheapen their own endeavors or call into question their sacrifice. And a

dlekauff, "Why Men Fought in the American Revolution," *Huntington Library Quarterly*, 3d ser., 43 (1980): 135-48. For examples in army thinking, see George C. Marshall, "West Point and the Citizen Army" (May 29, 1942), in Major H. A. De Weerd, ed., *Selected Speeches and Statements of General of the Army George C. Marshall* (Washington, 1945), 202; and General Bernard Rogers, *Cedar Falls-Junction City: A Turning Point*, Vietnam Studies (Washington, 1974), 159-60.

⁵ L. A. Pennington et al., *The Psychology of Military Leadership* (New York, 1943), 221, 222. Also see Hicken, *The American Fighting Man*, 57; Royster, *A Revolutionary People at War*, 219, 220-27; Major Edward Lyman Munson, Jr., *Leadership for American Army Leaders* (Washington, 1941), 88; and Colonel James A. Moss, *Officers' Manual* (8th ed., Menasha, Wis., 1941), 258-60, 286-87, 310-13. Note the ambivalence of a handbook prepared by a team made up mostly of scholars; see National Research Council, *Psychology for the Fighting Man* (Washington, 1943), 310-12. In World War II the army went to great lengths to indoctrinate soldiers, including commissioning the famous film series, "Why We Fight," by Frank Capra, and running essay contests among the soldiers on that theme. See Forrest C. Pogue, *George C. Marshall: Organizer of Victory, 1943-1945* (New York, 1973), 82-83, 84, 91-92; and Jack J. Zurofsky's essay in "Brooklyn Soldier Wins Essay Contest," *New York Times*, May 14, 1944, p. 14.

⁶ Marshall, *The Officer as a Leader* (Harrisburg, Pa., 1966), 268-76.

good deal of the literature and rhetoric has emanated from ethnic groups who have wanted to prove their loyalty and patriotism by portraying their participation in laudatory terms.

Thus the American soldier has been a symbol, a political and cultural artifact for a nation diverse in culture, uncertain in unity, and concerned through much of its history with proving its superiority to the rest of the world. Of necessity has been anonymity, which has further muddled the truth and contributed to the making of myths. Artists from the Revolution to the present have depicted enlisted men in faceless terms: Winslow Homer's *Harper's Weekly* drawings in the Civil War, for example, or Harvey Dunn's "The Machine Gunner" of 1918. Even characters with as much realism as Bill Mauldin's World War II GIs, "Willie" and "Joe," were meant to represent an anonymous mass of citizen soldiers.⁷ And the government—for recruiting, in bond drives, in memorials—has insisted on typicality, an "unknown soldier." From such sources the nation produced symbols that from the outset bore no necessary relationship to reality, and, because of anonymity and repetition generation after generation, the myths have survived with remarkable durability.

While no scholar has yet put together a synthesis of what is known about American soldiers, enough good studies and source material have become available to put finally to rest the popular legends. First of all, our forces have never been homogenous, but a bewildering mixture of ethnic and racial groups. Blacks served throughout American history, comprising, for example, as much as 20 percent of the Union Navy.⁸ Immigrants have also filled the ranks: over 20 percent of the Union Army, some 18 percent of the World War I draftees, and in peacetime in the nineteenth century even greater numbers—over 70 percent of army recruits in 1850 and 1851 and as late as 1894 one-quarter of enlisted ranks overall. In the navy, foreign-born sailors made up such a high percentage of enlisted ranks that officials wondered aloud whether the squadrons would be reliable in event of a crisis.⁹ Often the armed forces formed units from a single

⁷ For examples of battle art, see [Donald H. Mugridge, ed.] *An Album of American Battle Art, 1755-1918* (Washington, 1947); Julian Grossman, *Echo of a Distant Drum: Winslow Homer and the Civil War* (New York, n.d.); and the numerous photo histories of World War II. The anonymity of the enlisted man usually is portrayed explicitly in film documentaries, or specific individuals are offered up as "typical" of greater masses of men. For "Willie" and "Joe," see Bill Mauldin, *Up Front* (Cleveland, 1945).

⁸ Benjamin Quarles, "The Colonial Militia and Negro Manpower," *Mississippi Valley Historical Review*, 45 (1959): 643-52; Jack D. Foner, *Blacks and the Military in American History* (New York, 1974); and Joseph Pielucci, "Negro Sailors in the Union Navy, 1861-1865" (honors thesis, City College of the City University of New York, 1971), 17.

⁹ Benjamin Apthorp Gould, *Investigations in the Military and Anthropological Statistics of American Soldiers*, United States Sanitary Commission, *Sanitary Memoirs of the War of the Rebellion*, 2 (New York, 1869): 26-27; Bell Irvin Wiley, *The Life of Johnny Reb: The Common Soldier of the Confederacy* (1943; reprint ed., Garden City, N.Y., 1971), chap. 6; J. H. Baxter, *Statistics, Medical and Anthropological, of the Provost-Marshal-General's Bureau . . . during the Late War of the Rebellion . . .* (Washington, 1875), I, LXXIV-LXXV; Fred Davis Baldwin, "The American Enlisted Man in World War I" (Ph.D. dissertation, Princeton University, 1964), 59; Marcus Cunliffe, *Soldiers & Civilians: The Martial Spirit in America, 1775-1865* (Boston, 1968), 118-20; Francis Paul Prucha, *Broadax and Bayonet: The Role of the United States Army in the Development of the Northwest, 1815-1860* (Madison, Wis., 1953), 36, 41-45; Bruce White, "Ethnicity, Race, and the American Military, from Bunker Hill to San Juan Hill," in Major David MacIsaac, ed., *The Military and Society: The Proceedings of the Fifth Military History Symposium, United States Air Force Academy, 5-6 October 1972* (Washington, 1975), 122-25, 129-30; Harold D. Langley, *Social Reform in the United States Navy, 1798-1862* (Urbana, Ill., 1967), 38, 71-72, 89-92, 275; Frederick S. Harrod, *Manning the New Navy: The Development of a Modern Naval Enlisted Force, 1899-1940* (Westport, Conn., 1978), chaps. 1-2; and James E. Valle, *Rocks & Shoals: Order and Discipline in the Old Navy, 1800-*

group: the four regiments of blacks in the post-Civil War army, the American Indian regiments in the Civil War, the Indian companies in the 1890s, the Philippine scouts in the twentieth century, the Japanese-American battalions in World War II, and the Eskimo battalions of the Alaska Territorial Guard and Alaska National Guard, formed in World War II and still in existence today.¹⁰

Rarely have our forces constituted an economic or social profile of the general population. The Continental Army of the Revolution drew its soldiers from the poorest third of society and contained disproportionate numbers of drifters, servants, British deserters, captured loyalists, convicts, and drafted substitutes. Of the World War I draftees, three out of ten were illiterate or functionally so, five times the national illiteracy rate of 6 percent. In later conflicts men with college backgrounds or more education often became officers—in World War II, 40 percent of the men with some college background. (Of a sample of 231 select Harvard students and graduates, 80 percent held commissions.) The World War II ground armies could not have reflected the population, because so many of the skilled or technically apt were siphoned off into the air corps and navy. And our forces have rarely included another large group: the physically or mentally deficient. Selective Service rejected one quarter of those physically examined in World War II and nearly 50 percent of those eligible for the draft between 1950 and 1965.¹¹

1861 (Annapolis, 1980), 19–21. No one has, to my knowledge, compared the percentage of immigrants in enlisted ranks with the percentage of immigrants of the same age in American society. Since immigrants tended to be concentrated in the over twenty and under fifty age brackets, it is possible that immigrants were not as over-represented in the Army as is commonly believed.

¹⁰ White, "Ethnicity, Race, and the American Military," 113–30; Marvin Fletcher, *The Black Soldier and Officer in the United States Army, 1891–1917* (Columbia, Mo., 1974); Arlen L. Fowler, *The Black Infantry in the West, 1869–1891* (Westport, Conn., 1971); Bell Irvin Wiley, *The Life of Billy Yank: The Common Soldier of the Union* (1952; reprint ed., Garden City, N.Y., 1971), 316–19, and *The Life of Johnny Reb*, 324–27; Jack D. Foner, *The United States Soldier between Two Wars: Army Life and Reforms, 1865–1898* (New York, 1970), 129–31; Eric Fearer, "Indian Soldiers, 1891–95: An Experiment on the Closing Frontier," *Prologue*, 7 (1975): 109–18; Muktuk Marston, *Men of the Tundra: Eskimos at War* (New York, 1969); and Hicken, *The American Fighting Man*, 338–74. The armed forces have also included large numbers of aliens who have often used military service to gain American citizenship; see James B. Jacobs and Leslie Anne Hayes, "Aliens in the U.S. Armed Forces: A Historical-Legal Analysis," *Armed Forces and Society*, 7 (1981): 187–208.

¹¹ Mark Edward Lender, "The Enlisted Line: The Continental Soldiers of New Jersey" (Ph.D. dissertation, Rutgers University, 1975), iii–iv, 112–17, 122–27, 133–34, and "The Social Structure of the New Jersey Brigade: The Continental Line as an American Standing Army," in Peter Karsten, ed., *The Military in America from the Colonial Era to the Present* (New York, 1980), 27–44; John R. Sellers, "The Common Soldier in the American Revolution," in Stanley J. Underdal, ed., *Military History of the American Revolution: The Proceedings of the 6th Military History Symposium, United States Air Force Academy, 10–11 October 1974* (Washington, 1976), 151–56, 164–66; Edward C. Papenfuss and Gregory A. Stiverson, "General Smallwood's Recruits: The Peacetime Career of the Revolutionary War Private," *William and Mary Quarterly*, 3d ser., 30 (1973): 117–32; Baldwin, "The American Enlisted Man in World War I," 85–91; U.S. Bureau of the Census, *Historical Statistics of the United States, Colonial Times to 1970* (Washington, 1975), 382; Samuel Stouffer et al., *The American Soldier: Adjustment during Army Life*, Studies in Social Psychology in World War II, no. 1 (Princeton, 1949), 59, 245–47; Robert R. Palmer et al., *The Procurement and Training of Ground Combat Troops*, United States Army in World War II: The Army Ground Forces (Washington, 1948), 3–4, 6–10, 11, 16–17, 18; John Ellis, *The Sharp End: The Fighting Man in World War II* (New York, 1980), 10, 12, 192; John P. Monks, *College Men at War*, Memoirs of the American Academy of Arts and Sciences, vol. 24 (Boston, 1957), 286; U.S. Selective Service System, *Selective Service and Victory: The 4th Report of the Director of Selective Service . . .* (Washington, 1948), 147, 266, 666; Harry A. Marmion, "Historical Background of Selective Service in the United States," in Roger W. Little, ed., *Selective Service and American Society* (New York, 1969), 47; Charles Moskos, *The American Enlisted Man: The Rank and File in Today's Military* (New York, 1970), 41–43, 50–51, 64–74, 169–71, 195, 196; and Paul L. Savage and Richard A. Gabriel, "Cohesion and Disintegration in the American Army: An Alternative Perspective," *Armed Forces and Society*, 2 (1976): 345. For the unrepresentative character of the nineteenth- and twentieth-century peacetime army, see Cunliffe, *Soldiers & Civilians*, 116–20; Prucha, *Broadax and Bayonet*, 37–45; Don Rickey, *Forty Miles a*

Nor does the evidence support the belief that Americans enlist or fight purely out of patriotism. Often at the beginning of a war volunteers flocked to the colors in a surge of nationalistic fervor; invariably, however, the supply of willing recruits dried up as the realities of the sacrifice sank in. In the War of 1812 New England militia refused to turn out, and in 1814 the Madison administration considered introducing a national draft. In both the Revolution and Civil War, the states and the national government resorted to drafts. In both conflicts bounties were so pervasive and rose so high that they became a national scandal. Only in the Mexican and Spanish-American Wars has the nation manned its ranks without using large-scale bribery or coercion. And Americans have not submitted passively to coercion, except during World War II and the Cold War. When permitted in the Revolution and Civil War, thousands purchased substitutes. Others flouted the law. In World War I, 11 percent of those called evaded the draft, and nearly half of the evaders, some one hundred and seventy thousand men, were never apprehended. Long before Vietnam, Americans practiced medical chicanery. As one Civil War surgeon wrote in disgust, "They come fortified with elaborate certificates from sympathizing friends, kind-hearted family physicians, stupid quacks, and the learned homeopathist who has testified to the appalling infirmity of 'paralysis of the scrotum.'"¹²

Our peacetime forces have never depended on patriotism. Augustus Myers, who joined the Army fife and drum corps at the age of twelve in 1854 and who served ten years on the frontier and with the Army of the Potomac, remembered his comrades as enlisting "for various reasons. Some had the 'Wanderlust'; others . . . a taste for adventure. . . . Some had joined from sheer necessity, or inability to find any other occupation to support themselves, . . . a very common cause."¹³ In the nineteenth and early twentieth centuries men joined the peacetime army, and deserted, in direct correlation to pay and treatment inside the army and the state of the civilian economy outside. In fact, pay and bonuses have influenced American soldiers for generations. During the Revolution soldiers mutinied over lack of pay, and their officers three times threatened Congress with resignation unless granted half-pay pensions for life. Until the Cold War, the military pension system primarily benefited our "patriotic" citizen ar-

Day on Beans and Hay: The Enlisted Soldier Fighting the Indian Wars (Norman, Okla., 1963); Robert M. Utley, *Frontiersmen in Blue: The United States Army and the Indian, 1848-1865* (New York, 1967), 40-48; and Robert Kenaston Griffith, Jr., "The Volunteer Army and American Society, 1919-1940" (Ph.D. dissertation, Brown University, 1979).

¹² "Extracts from the report of Dr. J. F. Hall," 1st District New Hampshire, June 15, 1865, in Baxter, *Statistics, Medical and Anthropological*, 181. For the draft and bounties, see Marvin A. Kreidberg and Merton G. Henry, *History of Military Mobilization in the United States Army, 1775-1945* (Washington, 1955), 12-15; Lender, "The Enlisted Line," 78-82, 85-90; Eugene Converse Murdock, *Patriotism Limited, 1862-1865: The Civil War Draft and Bounty System* (Kent, Ohio, 1967), 13, 16-21; Russell F. Weigley, *History of the United States Army* (New York, 1967), 206-11; Edward M. Coffman, *The War to End All Wars: The American Military Experience in World War I* (New York, 1968), 28-29; Hicken, *The American Fighting Man*, 37; and Baldwin, "The American Enlisted Man in World War I," 25-28. One in five—20.8 percent—of those called under the North's draft refused to report; see Peter Levine, "Draft Evasion in the North during the Civil War, 1863-1865," *Journal of American History*, 67 (1981): 818-21.

¹³ Myers, *Ten Years in the Ranks U.S. Army* (New York, 1914), 37. Also see Hicken, *The American Fighting Man*, 47-50. The use of groups marginal on the social scale for peacetime forces went back to colonial times. See John Shy, *A People Numerous and Armed: Reflections on the Military Struggle for American Independence* (New York, 1976), 30-32.

mies, not the peacetime constabulary. The very first veterans' organization, the Society of the Cincinnati (for officers only), came into existence in 1783 in large part to press for promised pensions. By the end of the nineteenth century, veterans' lobbies had become such potent political forces that the government paid nearly five billion dollars in Civil War pensions alone. Not surprisingly, with over thirteen million men and women in uniform, the World War II Congress voted the most massive system of postwar benefits in history—before the fighting had even ended.¹⁴

Like soldiers of other nations, Americans have on occasion deserted, run from combat, murdered enemy soldiers, assaulted superiors, and wounded themselves to avoid battle. Rather than being motivated by patriotism or political ideology, our soldiers have sustained themselves in the stress of battle by a variety of means, from the network of esteem and interdependence that creates the solidarity and cohesion of a unit to the will and the instinct for survival and, perhaps, even to the unspoken belief in the superiority of their nation and culture.¹⁵ Away from the constraints of family and community, they have sworn, drank, gambled, fought among themselves, and chased women with enough regularity and in sufficient numbers to dispel any claim to special virtue. (In World War I the army stubbornly determined to stamp out venereal disease by prohibiting all fraternization with French women—and failed utterly.) Soldiers often expressed hate and contempt for enemy and ally alike. A few, on occasion, have been actually attracted to war, to the spectacle and to the excitement, as the philosopher J. Glenn Gray, who in 1940 received his doctorate and draft notice in the same mail, noted. And Americans have not always melted peacefully back into civilian life: they have often demonstrated to hurry demobilization, to receive bonuses, or to settle economic grievances, and they have organized themselves into vocal lobbies that, even today, exercise political influence far beyond matters of specific concern to veterans.¹⁶

¹⁴ See Foner, *Soldier between Two Wars*, 6–10; also see the studies on the nineteenth-century enlisted men cited in note 11, pages 557–58, above. For the correlation between peacetime recruiting or desertion and economic conditions or treatment inside the army, see Robert K. Griffith, Jr., "Quality not Quantity: The Volunteer Army during the Depression," *Military Affairs*, 43 (1979): 172–76, and "The Volunteer Army in American Society, 1919–1940," 72, 74–77, 81–83, 250–55. For veterans benefits, see William H. Glasson, *Federal Military Pensions in the United States* (New York, 1918), 273, 274; and Davis R. B. Ross, *Preparing for Ulysses: Politics and Veterans during World War II* (New York, 1969).

¹⁵ For the best studies on motivation in battle, see Edward Shils and Morris Janowitz, "Cohesion and Disintegration in the Wehrmacht in World War II," *Public Opinion Quarterly*, 12 (1948): 280–315; S. L. A. Marshall, *Men against Fire: The Problem of Battle Command in Future War* (New York, 1947), chaps. 9–11; Samuel Stouffer *et al.*, *The American Soldier: Combat and Its Aftermath*, Studies in Social Psychology in World War II, no. 2 (Princeton, 1949), chap. 3; Ellis, *The Sharp End*, chap. 8; Roger W. Little, "Buddy Relations and Combat Performance," in Morris Janowitz, ed., *The New Military: Changing Patterns of Organization* (New York, 1964), 195–223; John Baynes, *Morale—A Study of Men and Courage: The Second Scottish Rifles at the Battle of Neuve Chapelle, 1915* (New York, 1967); Charles Moskos, "Vietnam: Why Men Fight," in Martin Oppenheimer, ed., *The American Military* (n.p., 1971), 16–36, and *The American Enlisted Man*, chap. 6; and John Keegan, *The Face of Battle* (New York, 1976). For a recent review, see Kurt Lang, "American Military Performance in Vietnam: Background and Analysis," *Journal of Political and Military Sociology*, 8 (1980): 269–86. Alexis de Tocqueville, writing in the 1830s, thought Americans naturally undisciplined, but James Bryce, writing after the Civil War, concluded just the opposite; de Tocqueville, *Democracy in America*, ed. Phillips Bradley, 1 (New York, 1945): 191; and Bryce, *The American Commonwealth*, 2 (London, 1888): 442–43.

¹⁶ Jeffrey Geiger, "Morals, Venereal Disease, and the American Expeditionary Forces during World War I" (M.A. thesis, Queens College of the City University of New York, 1977); Baldwin, "The American Enlisted

THUS, AS MANY HISTORIANS HAVE LONG KNOWN, the record casts grave doubt on some of our most cherished folklore. But to portray the American soldier as a self-interested, blue-collar scoundrel motivated solely by money and survival, interested primarily in drink and in the pleasures of the flesh, liable to desert or succumb to his captor at the first opportunity, and kept in check only by the threat of army punishment would be as gross a distortion as the myth of the virtuous patriot. In recent years some of these images have dominated the popular mind: Korean POW's demoralized, disloyal, and collaborating; Vietnam veterans as murderous drug addicts incapable of shedding their alienation and returning to normal civilian lives.¹⁷ The problem, in both scholarship and popular thinking, is our propensity to search for typicality, to think in terms of stereotypes, and to aim for universal generalizations that fit across all of American history. The truth of the matter is that the "American soldier" never existed; the most pernicious myth of all is that there has ever been a prototypical American in uniform. Common sense ought to remind us that the past was different: the pace of life, values and attitudes, occupations, the classes and social structure of the nation, technology and the conditions of battle, the character of discipline, the nature of war and military life. Our experience as scholars should warn us that few, if any, generalizations for something as varied as military service, experienced by so many diverse individuals under such disparate circumstances, can hold over as long a span of time as three centuries. We ought particularly to suspect broad surveys in the absence of very detailed research on specific periods of time. And what we as historians should seek is not some set of large generalizations, but history—change over time, evolution, development.

Certainly many aspects of military organization and experience are universal and timeless; scholars, soldiers, and other writers often emphasize the similarities across national borders and centuries. Battle and the reactions of men caught in combat provides an excellent example of the tendency to generalize—and its pitfalls. John Keegan, a historian on the faculty at the Royal Military Academy at Sandhurst, wrote recently that what "battles have in common is human: the behaviour of men struggling to reconcile their instinct for self-preservation, their sense of honour and the achievement of some aim over which other men are ready to kill them." Studying battle "is therefore always a study of fear and usu-

Man in World War I," 33–41, 112–15, 184–90, 191–94; Hicken, *The American Fighting Man*, 193–205, 296–303; Frederick A. Pottle, *Stretchers: The Story of a Hospital Unit on the Western Front* (New Haven, 1929); Stouffer et al., *The American Soldier: Combat and Its Aftermath*, 156–57; Ernie Pyle, *Here is Your War: Story of G.I. Joe* (Cleveland, 1943), 145, 173–74; J. Glenn Gray, *The Warriors: Reflections on Men in Battle* (New York, 1959), 28–39; Moskos, *The American Enlisted Man*, 151; and Philip Caputo, *A Rumor of War* (New York, 1977), xv, 230. For veterans, see Dixon Wecker, *When Johnny Comes Marching Home* (Boston, 1944); Wallace Evan Davies, *Patriotism on Parade: The Story of Veterans and Hereditary Organizations in America, 1783–1900* (Cambridge, Mass., 1955); and Donald J. Lisio, *The President and Protest: Hoover, Conspiracy, and the Bonus Riot* (Columbia, Mo., 1974).

¹⁷ For American prisoners in the Korean War, see Albert D. Biderman, *March to Calumny: The Story of American POW's in the Korean War* (New York, 1963); and H. H. Wubben, "American Prisoners of War in Korea: A Second Look at the 'Something New in History' Theme," *American Quarterly*, 22 (1970): 3–19. For misrepresentations about Vietnam War veterans, see Jack Ladinsky, "Vietnam, the Veterans, and the Veterans Administration," *Armed Forces and Society*, 2 (1976): 435–67. And, for an example of negative generalizing over all of American history, see John Laffin, *Americans in Battle* (New York, 1973).

ally of courage; always of leadership, usually of obedience; always of compulsion, sometimes of insubordination; always of anxiety, sometimes of elation or catharsis; always of uncertainty and doubt, misinformation and misapprehension, usually also of faith and sometimes of vision; always of violence, sometimes also of cruelty, self-sacrifice, compassion; above all, it is always a study of solidarity and usually also of disintegration—for it is toward the disintegration of human groups that battle is directed.” Yet Keegan also insisted that battle belonged “to finite moments in history” and that its character depended heavily on the nature of the war, on the technology of the day, on the economies and societies of the opposing nations, on the armies, and even on the weather and the ground.¹⁸ In short, battle possesses a history of its own, an evolution over time that defines its dimensions and character.

Since battle has varied over hundreds of years, no single phenomenon could possibly explain the motives of soldiers, no matter how universal the emotions Keegan described. And yet many scholars have treated battle as a constant—have searched, with little regard for time and place, for the factors that explain why men fight. They have compared German soldiers in the latter stages of World War II with American soldiers in that war, and both of those with Americans in Korea and in Vietnam. Without doubt the phenomenon of primary group cohesion has, to some extent, been universal. Augustus Myers recalled that in the Civil War “the fear of the contempt of his comrades” was “even more powerful a factor than discipline in keeping a timid or nerveless soldier in the ranks during a battle.”¹⁹ In Vietnam, however, rotation loosened the ties of the unit by promising relief once the tour was over. One scholar has even argued that what sustained men in Southeast Asia was the understanding that only through collective action and cooperation could each individual hope to survive. The problem has been that the literature on primary group cohesion has never clearly shown whether solidarity with the group acted as a psychological prop to bolster men to endure the stress or as a motivation to carry out the mission and perform effectively in battle—or both. The same men in World War II who were supposedly welded together by the dynamics of the group were found by S. L. A. Marshall to have possessed an astonishingly poor frequency of firing their weapons at the enemy.²⁰

Perhaps the main reason for the inordinate focus on universal generalization has been that most of the literature has never sought to understand American soldiers in their own right. One category of study has aimed to celebrate or elucidate American character, another to establish ethnic or regional loyalty, a

¹⁸ Keegan, *The Face of Battle*, 297–98.

¹⁹ Myers, *Ten Years in the Ranks*, 254. For an example of attempting to extend modern sociological findings backwards, see Pete Maslowski, “A Study of Morale in Civil War Soldiers,” *Military Affairs*, 24 (1970): 122–26.

²⁰ See Moskos, *The American Enlisted Man*, 135, 144–46; John H. Faris, “An Alternative Perspective to Savage and Gabriel,” *Armed Forces and Society*, 3 (1977): 458–59; and Frederick J. Kviv, “Survival in Combat as a Collective Exchange Process,” *Journal of Political and Military Sociology*, 6 (1978): 219–32. Caputo noted a real difference in solidarity between the first Vietnam War marine battalions, in which the men enlisted, trained, and entered combat together in 1965, and later units, which operated under the rotation system; *A Rumor of War*, 32, 136–37, 155, 162–63.

third to criticize the government or its policies, usually by invidious comparison to past practice or success. For all of these purposes, writers of necessity lumped together wars and centuries, for bravery or cowardice could never be demonstrated convincingly except over a large span of American history. Much of the literature, in addition, has come from social scientists bent not necessarily on understanding soldiers but on advancing scholarly methodology, or serving the government in its practice of "human engineering," as psychologist Robert M. Yerkes called manpower policy in 1941.²¹ In 1840 the surgeon general of the army collected statistics on sickness in order to improve health practices in the service.²² After the Civil War both the U.S. Sanitary Commission and the Provost-Marshall-General's Office published compilations of the physical and sociological characteristics of recruits and draftees to provide information about the American population.²³ The most famous study ever done on American enlisted men, the 1949 multi-volume classic, *The American Soldier*, based on extensive interviews with the troops during World War II and the product of sophisticated analysis by a team of outstanding scholars, was promoted by its publisher and acknowledged by its authors primarily for its breakthrough in social scientific method and for its revelations about human behavior, not for its information about military life or soldiering.²⁴ The 1970 presidential commission on an all-volunteer armed force operated under a charge by the president not to study soldiers but "to develop a comprehensive plan for eliminating conscription and moving toward an all-volunteer Armed Force."²⁵ Virtually the entire literature on the volunteer army debate of the last decade and a half has treated the American soldier as an object, a unit of labor, an "asset" without humanity in any historical sense.²⁶ Even as solid a study as *The Ineffective Soldier*, by scholars

²¹ Yerkes, "Man-Power and Military Effectiveness: The Case for Human Engineering," *Journal of Consulting Psychology*, 5 (1941): 205-09.

²² Thomas Lawson, *Statistical Report on the Sickness and Mortality in the Army of the United States . . . from January, 1819, to January, 1839* (Washington, 1840), title page, ii-iv, 5, 43.

²³ Gould, *Investigations in the Military*, vi, *passim*; and Baxter, *Statistics, Medical and Anthropological*, i, iii, vii, *passim*.

²⁴ Samuel A. Stouffer, "Some Afterthoughts of a Contributor to 'The American Soldier,'" and Daniel Lerner, "The American Soldier and the Public," in Robert K. Merton and Paul F. Lazarsfeld, eds., *Studies in the Scope and Method of "The American Soldier"* (Glencoe, Ill., 1950), 9, 10, 198, 200-01, 216-19, 232-33; and Robert S. Lynd, "The Science of Inhuman Relations," *New Republic*, August 29, 1949, pp. 22-25.

²⁵ [Thomas Gates et al.] *The Report of the President's Commission on an All-Volunteer Armed Force* (Washington, 1970), vii, 6-7. The bias of the commission was also revealed in the *Studies Prepared for the President's Commission on an All-Volunteer Armed Force*, 2 vols. (Washington, 1970). For the arguments over the all-volunteer army and the background of the Gates Commission, see Gus C. Lee and Geoffrey Y. Parker, *Ending the Draft—The Story of the All Volunteer Force, HumRRO Final Report 77-1* (Alexandria, Va., 1977), 24-60.

²⁶ Charles C. Moskos, Jr., noted the dominance of economic models; see his "Statement . . .," *Status of the All-Volunteer Armed Force: Hearing before the Subcommittee on Manpower and Personnel of the Committee on Armed Services, United States Senate*, 95th Cong., 2d session, June 20, 1978 (Washington, 1978), 38, 41, 47-48. The all-volunteer army debate began in the mid-1960s and focused first on the inequities of the draft, then later in the decade more on the feasibility of adequately manning such a force. Once the draft ended and acceptable force levels seemed at least a possibility, emphasis seemed to shift more to the composition and the quality of the soldiers of an all-volunteer army. Arguments over attracting volunteers and over their fitness for service were those most clearly slanted toward the soldier as a unit of labor. Very little of the literature provided any historical background or perspective, and, significantly, much of it used selected (and often distorted) historical facts to buttress policy recommendations. For good examples of such flaws, see [Gates] *Report of the President's Commission*, 5-6, 14, 130-31, and John L. Rafuse, "United States' Experience with Volunteer and Conscript Forces," in *Studies Prepared for the President's Commission*, 3.1: 1-46. Naturally, the Gates Commission argued that the all-volunteer principle was traditional. Ironically, the Selective Service, trying to justify permanent peacetime

comprising the Columbia Conservation of Human Resources Project in the 1950s, endeavored chiefly to broaden "knowledge about the nation's human resources" for "better development and utilization of this most valuable resource."²⁷

Most of these works possess great value for scholars, and in the last few years some excellent histories of enlisted men have come forth. For example, the army on the nineteenth-century frontier has been thoroughly reconstructed, down to specific studies of the black units and of the women and children who accompanied the men out West.²⁸ Yet our knowledge remains piecemeal, tiny in proportion to the vastness of the subject; and, because of the drive for generalization and neglect of change over time, we have hardly begun to address the more difficult—and most revealing—issues raised by military service.

IT IS TIME FOR HISTORIANS to take a fresh look at the American soldier; for too long, political and policy concerns have dictated the very categories of inquiry. As we begin, we need to remind ourselves that the past in many respects did not resemble the present, that youth, battle, warfare, and enlisted life may have changed significantly since the seventeenth century. We ought also to cast a suspicious eye on old assumptions—for example, that military institutions reflect society. The army never reflected American society, unless a centralized, stratified, cohesive, authoritarian institution that has stressed obedience and sacrifice can reflect a decentralized, heterogeneous, individualistic, democratic, capitalist society. If our military forces at any given time have reflected American values and practices, it has only been in comparison to the forces of other nations or if measured against some sociological model of an ideal military organization. We should remember that there have been many different military forces in our history, most of which changed over the course of their existence, and rarely, if ever, comprised a representative cross-section of the American population. Peacetime constabularies and wartime armies have been organized for very different purposes and have been manned by very different types of individuals. Sometimes, the composition of forces differed radically; for a single Indian war in the early 1790s, the government raised three separate armies made up of mixtures of militia, militia substitutes, six-month volunteers, thirty-day volunteer cavalry, and regulars. Some services or units, like the navy and air corps, have purposely drawn on more educated and skilled groups. And through our history officers have almost always come from the wealthier or better educated segments of society, and at the other extreme large numbers of Americans have of-

conscription after World War II, argued that compulsion was both traditional and most consonant with American political ideals. See U.S. Selective Service, *A Historical Review of Citizen Compulsion in the Raising of Armies*, Backgrounds of Selective Service, Special Monograph no. 1 (Washington, 1947).

²⁷ Eli Ginzberg *et al.*, *The Ineffective Soldier: Lessons for Management and the Nation*, 3 vols. (New York, 1959), 3: 9. Also see *ibid.*, 1: vii-x, xvii-xx, 1-2, and 3: xvii-xix, 3-5, 8-13, 23-24.

²⁸ For the nineteenth-century army, see the works cited in notes 10-11, pages 557-58, above; and Patricia Y. Stallard, *Glittering Misery: Dependents of the Indian Fighting Army* (San Rafael, Calif., 1978).

ten been excluded by reason of race or for physical, mental, or moral deficiency.²⁹

In essence, the task will be to construct a history of military service in the United States. Once the experience of service returns to the proper context of time and place, the American soldier will begin to gain an understandable identity. At least three lines of inquiry need to be pursued, each of which will require the same methods, and the same sensitivity and imagination, that historians now apply to the study of other groups of the "inarticulate," or nonelite masses. Soldiers fortunately have been most articulate. Because they usually left home, they often wrote letters or kept diaries to remember an experience that was so important and so unusual in their lives. A few wrote memoirs or autobiographies. But historians have relied too heavily on the rich literary record, forgetting that the literate few and their perceptions may not have been representative. To recapture the great multitude there exists another documentary record: the mass of enlistment papers, muster rolls, returns, payrolls, court-martial records, hospital rolls, pension files, unit books, personnel files, administrative reports, correspondence, and other materials about military service in the National Archives and in some state archives and private manuscript repositories.³⁰ To use these sources scholars must bridge the traditional boundaries between military history and other professional specialties and use the tools of other disciplines.

First, historians must discover who served, who enlisted in a community and who did not, whom the draft caught and who escaped: their age, ethnic background, wealth, occupation, length of time in the community, and whatever additional information can be gathered or wrung indirectly out of the sources. Except for a few case studies, this basic spadework has never been done, and, until it is, any theories or generalizations about soldiers will not be persuasive. Further, understanding the true identity of the soldiers means grounding them in the communities and times in which they lived. From the profusion of community studies, historical geographies, studies of ethnic groups and urban neighborhoods and industrializing towns that has poured forth in recent years and from older local histories, scholars can begin, for a particular age or group of enlisted men, to establish a benchmark from which to trace the nature and impact of

²⁹ For various armies, see Mark E. Lender, "The Mind of the Rank and File: Patriotism and Motivation in the Continental Line," in William C. Wright, ed., *New Jersey in the American Revolution III: Papers Presented at the Seventh Annual New Jersey History Symposium* . . . (Trenton, N.J., 1976), 22-25, and "The Enlisted Line," viii, chap. 3; Richard H. Kohn, *Eagle and Sword: The Federalists and the Creation of the Military Establishment in America, 1783-1802* (New York, 1975), chaps. 6, 8; Wiley, *The Life of Johnny Reb*, 123-27, and *The Life of Billy Yank*, 343-44; Baldwin, "The American Enlisted Man in World War I," 85; and Palmer et al., *Procurement and Training of Ground Combat Troops*, 14-86. One good example of denial of service occurred in the Spanish American War, when the flood of men enabled the regular army to reject three out of four volunteers; Gerald F. Linderman, *The Mirror of War: American Society and the Spanish American War* (Ann Arbor, 1974), 61-64.

³⁰ See the *Guide to the National Archives of the United States* (Washington, 1974). For an excellent example of the range of sources available, published and in manuscript, for one war, see J. Todd White and Charles H. Lesser, eds., *Fighters for Independence: A Guide to Sources of Biographical Information on Soldiers and Sailors of the American Revolution* (Chicago, 1977).

military service.³¹ The conditions of childhood and youth and the history of education also deserve attention, for how children have been raised and taught, disciplined or neglected, and supported or sent out into the work force undoubtedly affected their adaptation to military life. Perhaps the substitution in the twentieth century of separate basic training for training in the regiment stemmed as much from the creation of the "adolescence" stage in youth and the onset of a youth culture removed from the adult world as from the need for military efficiency or the demands of military technology.³² The treatment of youth might have varied by region or ethnic background, affecting soldier behavior. Historians must find all of this out. In addition, they ought to study the world of work and leisure that has begun recently to attract scholarly attention. In the end, how young men integrated into the military and how they behaved in camp and field undoubtedly depended on the culture and values they brought with them.³³ Without knowing the specific worlds they left, scholars cannot fully comprehend American enlisted men beyond the stereotypes offered up by observers since the time of von Steuben and de Tocqueville.

Second, historians must undertake a new look at military service itself by reconstructing the life and environment of the enlisted man in much greater detail and depth than has ever before been attempted. The method could be that of the *Annales* school of French historiography. *Annales* historians pursue a "total" history: the recreation of the entire world of an age or group, the totality of life, through a consideration of climate, geography, vital statistics of the life cycle, migration, architecture, economic activity, and all of the myriad facets of life in the past.³⁴ By first asking the most basic descriptive questions and using the methods of other disciplines, military historians can recover the fullness of the military experience. Initial research might concentrate on the assumptions that the military has brought to the acquisition, training, and government of

³¹ Historians might wish, for example, to study a group of soldiers, sailors, volunteers, or militiamen from a particular area or town for which a demographical, geographical, or sociological study has already been done. The alternative is to reconstruct the needed elements of wealth, social structure, mobility, and the like, as Mark Lender has partially done for "The Enlisted Line."

³² The following were suggestive along these lines: Philip Greven, *The Protestant Temperament: Patterns of Child-Rearing, Religious Experience, and the Self in Early America* (New York, 1977); John R. Gillis, *Youth and History: Tradition and Change in European Age Relations, 1770-Present* (New York, 1974); and Joseph F. Kett, *Rites of Passage: Adolescence in America, 1790 to the Present* (New York, 1977).

³³ For a similar point, see Eric J. Leed, *No Man's Land: Combat and Identity in World War I* (Cambridge, 1979), 8-14. For an excellent study of the linkage between the civilian world and military service, see Linderman, *The Mirror of War*, chaps. 3-4. And, for a brief, but very suggestive, example of how the total reconstruction of life in a region or community can illuminate the study of enlisted men and suggest ways military service affected the men, see Eugen Weber, *Peasants into Frenchmen: The Modernization of Rural France, 1870-1914* (Stanford, 1976). In a short chapter entitled "Migration of Another Sort: Military Service," Weber has argued that "the Army turned out to be an agency for emigration, acculturation, and in the final analysis, civilization, an agency as potent in its way as the schools"; *ibid.*, 302. Weber's suggestive discussion emphasizes some of the differences between life in service and the peasant civilian world.

³⁴ My understanding of the *Annales* school comes essentially from Fernand Braudel, *The Mediterranean and the Mediterranean World in the Age of Philip II*, trans. Siân Reynolds, 2 vols. (2d French ed., 1966; New York, 1972), *Capitalism and Material Life, 1400-1800*, trans. Miriam Kochan (1967; New York, 1973), and "Personal Testimony," *Journal of Modern History*, 44 (1972): 448-67; H. R. Trevor-Roper, "Fernand Braudel, the *Annales*, and the Mediterranean," *ibid.*, 468-79; J. H. Hexter, "Fernand Braudel and the *Monde Braudellien* . . .," *ibid.*, 480-539; and Trián Stoianovich, *French Historical Method: The Annales Paradigm* (Ithaca, N.Y., 1976).

soldiers, then the regulations of armies and navies, the uniforms and the equipment, the drill and fatigue duty—in short, the minutiae of military life in its entirety. Using such an approach as well as sources long extant but rarely used for this purpose, historians could create so realistically the physical and mental world of the enlisted man that the time-honored reliance on literary sources and on the artistry of fiction could be diminished. Startling new material might emerge on such topics as the large and important role of women in military organization and war before the twentieth century.³⁵ Scholars will need to know not only the traditional areas of what soldiers thought about war, their comrades and officers, the government, service life, and the world around them, but how they thought and behaved, especially in comparison to others of their time and background outside the military.³⁶ Historians might also borrow insights from the approach of anthropology and sociobiology, not only for the study of cohesion in combat but also for older areas of concern such as training and officer-enlisted relationships.³⁷ Another and more complete look at military service will permit testing long-held notions about military isolation from American life and the reciprocal effect of the military on Americans and they on the character of our military institutions.³⁸

The interaction between the military and the rest of society should form a third category of research. Historians might begin with the exit of soldiers from their communities, how the process of providing men and their leaving affected local life. The absence of the men, the burial of the dead, and the return of the wounded also deserve attention, for how a town or region did or did not adjust to the losses or embrace the sick and maimed could reveal much about war and American culture. For this purpose, the role and behavior of women—especially in areas where large numbers of men left—will be crucial. The re-entry of veterans must also be examined to learn the impact of service and homecoming: the changes in the status of veterans compared to those who did not serve, adjust-

³⁵ Linda Grant De Pauw, "Women in Combat: The Revolutionary War Experience," *Armed Forces and Society*, 7 (1981): 209–26. Also see John Todd White, "The Truth about Molly Pitcher," in James Kirby Martin and Karen R. Strubaus, eds., *The American Revolution: Whose Revolution?* (Huntington, N.Y., 1977), 99–105.

³⁶ A large body of sources that historians have not exploited are medical records, such as the statistics of disease and disability, and, for the twentieth century, psychiatric records. See, for example, for World War I, Elmer E. Southard, *Shell-Shock and Other Neuropsychiatric Problems Presented in Five Hundred and Eighty-Nine Case Histories from the War Literature, 1914–1918* (Boston, 1919); and U.S. Surgeon General's Office, *Neuropsychiatry in the United States, in the American Expeditionary Forces, The Medical Department of the United States Army in the World War*, 10 (Washington, 1929).

³⁷ Without either endorsing or rejecting the assumptions, conclusions, and claims of sociobiology, I nonetheless believe historians of enlisted men, particularly those interested in behavior in battle, would benefit from the questions and style of analysis of the field, especially the emphasis on male relationships, aggression, and altruism. For the approach from sociology and anthropology, see Lionel Tiger, *Men in Groups* (New York, 1969); and Lionel Tiger and Robin Fox, *The Imperial Animal* (1971; reprint ed., New York, 1974). For the approach from the sciences, see Edward O. Wilson, *Sociobiology: The New Synthesis* (Cambridge, Mass., 1975), particularly chaps. 1, 2, 5, 11, 27, and *On Human Nature* (Cambridge, Mass., 1978); and N. Tinbergen, "On War and Peace in Animals and Man," *Science*, 160 (1968): 1411–18. For a briefer introduction, see D. P. Barash, *Sociobiology and Behavior* (New York, 1977); and, for a good compilation of the controversy over the field, see Arthur L. Caplan, *The Sociobiology Debate: Readings on the Ethical and Scientific Issues concerning Sociobiology* (New York, 1978). For a wise and perceptive review, see the late Charles Frankel's "Sociobiology & Its Critics," *Commentary*, July 1979, pp. 39–47.

³⁸ The isolation theme has undoubtedly been overemphasized. See, for example, John M. Gates, "The Alleged Isolation of US Army Officers in the Late 19th Century," *Parameters*, September 1980, pp. 32–45.

ments in social structure or political life, the importance of veterans' organizations, the geographic as well as economic mobility of veteran populations, and the like. In every instance scholars should investigate government policy, particularly its effects, for increasingly through American history, national and state benefits for veterans—from bounty lands in the Revolution to Civil War pensions to educational grants and employment preferences for the soldiers of World War II—shaped American society.³⁹

Certainly no single scholar will be able to create a complete portrait of the American soldier; such a history will probably emerge gradually from the work of many, each of whom may choose to focus on one or several aspects of what promises to be a very wide area of inquiry. Nor should military historians necessarily dominate in this quest, for, although the focus may rest on military subjects, the research and the knowledge to be gained ranges far away from what has traditionally been considered "military history." Social historians have much to gain from such rich records and also much to learn, for military service resides properly within the broader history of American society and may reveal much about nonmilitary subjects.⁴⁰ Beginnings, of course, have been made and research is going forward. But until the outlines of a history emphasizing change over time become clear, a crucial portion of the American past will remain hidden, the subject perhaps no longer of old myths but the subject, equally dangerous, of shaky generalizations based on limited case studies. After so much time, and so many books, the great difficulty will be avoiding those generalizations.⁴¹

Until we, as historians, abandon both our stereotypes and our propensity to think in terms of stereotypes, the American enlisted man will remain as anonymous as the unknown soldier in the tomb at Arlington National Cemetery. For reasons of state, the tomb will continue to serve as a symbol of sacrifice and the gratitude of a nation. For the historical profession, however, the unknown soldier poses a special challenge: a sound, impartial rendering of the past.

³⁹ There is, of course, a large literature on veterans, veterans' organizations, and demobilization, almost none of which focuses in any depth or detail on a specific group of men or local communities.

⁴⁰ For an example suggesting such possibilities, see Roger Norman Buckley, *Slaves in Red Coats: The British West India Regiments, 1795–1815* (New Haven, 1979).

⁴¹ The best general review of the literature on the American soldier is Karsten, *Soldiers and Society*, 3–48. Karsten, however, arranged his source readings topically and argued by implication for generalization across time: "I was struck by the degree to which the experiences and attitudes of eighteenth-, nineteenth-, and twentieth-century American soldiers were more alike than they were different. . . . there were differences But on balance, the differences appear (to me, at least) to be less significant than the similarities." *Ibid.*, 12. I disagree, particularly on the methodological grounds argued in this essay.

Reviews of Books

GENERAL

ROBERT NISBET. *History of the Idea of Progress*. New York: Basic Books. 1980. Pp. xi, 370. \$16.95.

Robert Nisbet has written an unblushing paean to the idea of progress and a lament, almost a threnody, for its disintegration in the twentieth century. "This history of all that is greatest in the West," he affirms, "... is grounded deeply in the belief that what one does in one's own time is at once tribute to the greatness and indispensability of the past, and confidence in an ever more golden future" (p. 8). Since the idea of progress, in turn, emanates from "a deep and wide sense of the sacred" (p. 357), and since contemporary intellectuals by and large have abandoned any such sense, Western civilization is quite possibly scheduled for a long season of chaos and perhaps *Untergang*.

To argue his case, Nisbet takes his readers on a tour of Western thought since Hesiod. He achieves a terrific velocity, as anyone must do on journeys of three millennia in length. Occasionally playing fast and loose with evidence wrenched out of context, he attempts to show that ancient and medieval culture in the West was suffused with a sturdy and world-affirming belief in progress, incorporating all the elements of modern progressivism. After the brief interruption of the so-called Renaissance, when a sour-minded cyclical view of history prevailed, the Puritans took up the good cause again.

In the modern world proper, the world of 1750 to 1900, Nisbet argues that the idea of progress dominated all departments of thought. He divides modern visions of progress into two sorts: the idea of progress as freedom, of which he enthusiastically approves, and the idea of progress as power, seen as a "corruption" of the true faith, albeit generally well meaning and no less passionate. Nisbet's major apostles of the liberal idea are Turgot, Smith, Jefferson, Condorcet, Godwin, Malthus, Kant, Mill, and—"grand culmination and fulfillment" (p. 236)—Herbert Spencer. Those who preached human redemption through power, in his reckoning, include Rousseau, Fichte, Hegel, Saint-Simon, Comte, Marx, and Gobineau. In both the true and

the corrupt gospels of progress Nisbet finds that religious inspirations were decisive, no matter how "secularized" a thinker claimed or appeared to be. Two final chapters study the dismal fortunes of the idea of progress in the twentieth century, drawing on an odd assortment of mostly quite recent and mostly quite minor sources.

Despite the annoying absence of page references or a bibliography, Nisbet's volume does marshal with some effectiveness the arguments for the flowering of an idea of progress before modern times and for the interconnectedness of progressivism and religion. But his weakly developed sense of changing historical milieux and his intense partisanship prevent him from writing a definitive work. J. B. Bury is challenged but not overthrown.

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KARL HEINZ METZ. *Grundformen historiographischen Denkens: Wissenschaftsgeschichte als Methodologie; Dargestellt an Ranke, Treitschke und Lamprecht; Mit einem Anhang über zeitgenössische Geschichtstheorie*. (Münchener Universitäts-Schriften, Reihe der Philosophischen Fakultät, number 21.) Munich: Wilhelm Fink Verlag. 1979. Pp. 737. DM 160.

This ambitious work—an unrevised dissertation—is daunting in several respects, not the least of which is its massive size. Its thesis is that historians who forget their own historiographical past are condemned to rewrite history, usually as fruitlessly twice-told tales. In other words, the author believes that a critical consciousness of past historical writing is necessary to advance current practice and to bring history into closer working relations with the social sciences as well as political philosophy. A sustained and searching dialogue with three past masters—Ranke, Treitschke, and Lamprecht—becomes the vehicle for examining three different methods that have, with modification, been carried over into the present. These methods are considered "basic" yet amendable.

The structure of Metz's book is unusual and

clearly helpful in a work of this size and complexity. There are parallel numbered sections for each figure, so that one can conveniently refer across topics such as language, narrative technique, conception of science or of action, and so on. The reader is provided with extensive treatment of the life, writings, followers, and critics of each figure, along with their immediate and long-term reputations. Although insisting that there can be no sharp separation of method and general historical perspective, Metz emphasizes the former through painstaking expositions of specific texts, such as Ranke's English history, Treitschke's essay on Bonapartism, and portions of Lamprecht's enormous German history. The attempt at "comparative statistical stylistics" turns up some interesting results in tabular form. On the other hand, the author's charts and diagrams (complete with formulas, sub- and superscripts) seem rather factitious. The notes and index are excellent, although the bibliography slights some recent theory-oriented work in English, such as Leonard Krieger's study of Ranke and Karl Weintraub's *Visions of Culture* on Lamprecht.

Metz's three historians represent different tendencies of inquiry: Ranke the objective (*gegenstandsbezogene*), Treitschke the "present-centered" (*gegenwartsbezogene*), and Lamprecht the law-centered or nomothetic (*gesetzesbezogene*). Each historian emerges as a theorist of his craft—sometimes almost in spite of himself. Ranke stands forth as a front-rank political thinker whose notion of universal history was not merely a postulate tacked onto his specialized histories. Treitschke receives a new hearing on matters such as criteria of relevance, the role of elites in politics, and what could be termed the rhetoric of history. Lamprecht's program of cultural history, after much denigration at home, found its continuation abroad in the "new history" and *Annales* school. On a broad front Metz is reminding present-day readers of what Ranke once said of himself to Wilhelm Scherer: "I am altogether more original than you think."

Metz's book suffers from a certain theoretical overkill characteristic of recent German scholarship. The repeated digressions and regressions (to Polybius, to Plato's cave, to Hobbes's behemoth) do not cohere sufficiently. There are too many long, dry stretches of backtracking erudition between oases of plain speaking. Admittedly it is not easy to talk about the uses of type, comparison, analogy, law, and so on, but what is served by swimming after every new wave, whether cybernetics, game-theory, or linguistics? Metz's text is marred by prolixity and neologisms, such as "histomat" (historical materialism), "PH models" (Popper-Hempel), and "operationalizability." It could be cut to half its size without real damage to substance or style.

This work demonstrates that theoretical analysis

of historical writing has advanced to a high plane of refinement. Whether this kind of methodological, stylistic, and linguistic self-consciousness will result in better actual history remains to be seen.

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DANIEL J. WILSON. *Arthur O. Lovejoy and the Quest for Intelligibility*. Chapel Hill: University of North Carolina Press. 1980. Pp. xvii, 248. \$18.00.

The discipline of intellectual history owes a considerable debt to the philosopher Arthur O. Lovejoy. In addition to being one of the founders of *The Journal of the History of Ideas*, he was an influential advocate of studying ideas as fundamental components of the historical process. A masterful prototype was Lovejoy's own *Great Chain of Being*, published in 1936, in which the Johns Hopkins scholar traced a significant "unit-idea" from Plato through the development of Western thought. While few intellectual historians have attempted to match Lovejoy's pioneering example, they have generally acknowledged his importance in giving their specialty professional legitimacy. Yet, this was but a part of a larger effort at understanding the universe. Daniel J. Wilson has aptly characterized his study of Lovejoy as a quest for intelligibility.

In what might be fairly described as an intellectual biography, Wilson has examined Lovejoy's writings about religion, his critiques of pragmatism and idealism, his defenses of critical realism and epistemological dualism, as well as his forays into the history of ideas and has uncovered, he believes, the central theme to Lovejoy's frequently complex thought. The academic philosopher found in human reason, in precise patterns of thought, and in an acceptance of temporalism the basic devices necessary to comprehension and knowledge. Wilson is best in explicating Lovejoy's dissection of William James's pragmatism and Henri Bergson's temporalism but weaker when attempting to adumbrate the philosopher's vigorous defense of critical realism. Yet Wilson has achieved a satisfactory overview of Lovejoy's thought—one based upon a thorough examination of manuscript as well as published sources.

In addition to discussions of Lovejoy's philosophical, religious, and historical thinking, the book contains interesting accounts of the scholar's troubled childhood, of the frequent religious disagreements with his father, of his academic career and role in the founding of the American Association of University Professors, and of his work as a propagandist during World War I. Indeed, a major part of the book is devoted to describing Lovejoy's consistent social and political activism, which was based upon his total commitment to the free marketplace of

ideas. Wilson has not written an uncritical apologia, however. He is careful to note inconsistencies between precept and performance, and he admits the diminishing influence of his subject. What he has accomplished is a scholarly and thorough study of a significant American thinker and a representative figure of the times in which he lived.

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HUGH RAGSDALE. *Détente in the Napoleonic Era: Bonaparte and the Russians*. Lawrence: Regents Press of Kansas. 1980. Pp. xii, 183. \$17.50.

Hugh Ragsdale's linguistic and scholarly talents have enabled him to produce an exceptional book. Almost no other specialist in revolutionary-Napoleonic studies could have used the documents he did—in both French and Russian, as well as in Danish, Swedish, German (for Austria and Prussia), and English. His bibliography is heavy on archival material and, with his notes, almost overwhelms the text. The narrative is not dull, however, but spare, well honed, and lucid; his deft characterizations give it spice.

The core of the monograph centers on 1799–1801, when Tsar Paul informally allied himself with the new first consul of France, Napoleon Bonaparte. This, Ragsdale proves handily, had more to do with the general peace that ensued—the first in a decade—than did Napoleon's crossing of the Alps and victory at Marengo or Moreau's victory at Hohenlinden.

Paul (the "mad tsar," by many accounts) played a mean game of power politics. He threatened Austria from the east (when it had already been beaten by France); Austria gave Bonaparte all too much for peace at Luneville. Paul formed a League of Armed Neutrality (Denmark, Sweden, Prussia) around the Baltic that stopped British commerce dead. Britain ousted the Pitt cabinet and sued for peace, which was made on unfavorable terms at Amiens. (Nelson, meanwhile, disdaining his superior's orders, had blown the Danish fleet out of the water at Copenhagen and opened the Baltic, but it did not matter.)

The Franco-Russian arrangement ended with the assassination of Paul in 1801. Nevertheless, Paul had shown Napoleon the one means by which he might control Europe—alliance with Russia. Ragsdale demonstrates, however, that Bonaparte probably could not have sustained the "alliance" with Paul for long—or any later similar arrangement. Bonaparte was dealing honestly with Paul in northern Europe but practicing duplicity with respect to Italy, Malta, and the future of the Ottoman empire. His behavior, in short, forecast the nature of his dealings with Alexander I after 1807. Napoleon had

to "have it all"; to share power in Europe was against his nature.

It is baldly evident from the above that both the book's title and subtitle are misleading. It is not about "détente," and it does not cover the whole of Napoleon's relations with the Russians. The book is nonetheless valuable for what it is, and I blame the mistitling on the present-minded atmosphere in which its publication was negotiated.

Ragsdale is the best qualified scholar I know to write the "rest of the story" on the relations between Bonaparte and Alexander I. I hope we will have another fine book from him on that subject soon.

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STEPHEN J. WHITFIELD. *Into the Dark: Hannah Arendt and Totalitarianism*. Philadelphia: Temple University Press. 1980. Pp. xii, 338. \$18.95.

Hannah Arendt remains a perplexing, and sometimes disturbing, star in the galaxy of contemporary political thinkers. She did not fit comfortably in any intellectual or political camp, school, or fashion. The Jewish refugee from German oppression confessed she had no particular "love of the Jewish people" and that she came out of the tradition of German philosophy. The implications of her distaste for modern culture, her emphasis on politics as concerned with creating and founding, her stress on active participation in the public realm as the exemplification of freedom, the communal nature of consciousness in her theory, the exaltation of ancient Greece as a political model—all this troubled many who assumed that the brilliant critic of the perverse Nazi and Stalinist regimes would be a defender of liberal individualism and representative democracy.

In his measured and temperate book Stephen J. Whitfield sees Arendt as imbued with intellectual passion, full of fresh and unusual illuminations and dark flashes of insight. He has not written a biography of Arendt but has provided a valuable assessment of her explanation of totalitarianism at a moment when that term is not universally accepted as valuable. He is not the first critic to do so, but he is particularly lucid and fair in his assessment, especially when dealing with her controversial discussion of Eichmann.

Whitfield sees Arendt's work as constituting the most convincing single effort to grasp the meaning of the Nazi and Stalinist regimes. While admiring that effort Whitfield examines the cohesiveness of her theory and its coincidence with the reality. He is more successful in this than in his attempt to place Arendt's interpretation of totalitarianism within the context of her general political thought or her views on authority and revolution.

Arendt viewed totalitarianism as an unprece-

dented phenomenon, the combination of ideology and terror that wanted to alter the human condition, to destroy individuality, to implement "the logic of an idea" by revising reality and destroying facts and people that might oppose the ruling group. Beginning with an analysis of antisemitism, which Whitfield sees as central to her thesis, Arendt regarded the irrational fears of Jews as integral of the totalitarian mind.

Whitfield cogently indicates some limitations and ambiguities in her analysis. Arendt paid scant attention to the personalities of the dictators. She underestimated the significance of nationalism. She says little on the controversial question of whether Lenin's rule, and Marxism in general, led inexorably to Stalinism. Her view that totalitarianism resulted from mass society with its lack of integration, weak sense of a collective entity, loneliness, and sense of authority is more applicable to Germany than either to backward Russia, which became totalitarian, or to the developed West which did not.

Whitfield has discerned more inconsistencies in Arendt's writings than is customary. One appreciates these all the more in view of his high regard for her work. It is also fitting for a teacher of American studies to miss in Arendt a fuller appreciation of Anglo-American liberalism, the real opponent of totalitarianism.

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JAMES J. BARNES and PATIENCE P. BARNES. *Hitler's Mein Kampf in Britain and America: A Publishing History, 1930-39*. New York: Cambridge University Press. 1980. Pp. xiii, 157. \$18.95.

A book as momentous as *Mein Kampf* deserves to be examined in virtually every way possible. James J. Barnes and Patience P. Barnes have worked painstakingly, in an extraordinarily wide range of sources, to round out our knowledge of that work. The result is this attractively produced, well-written account.

Not until October 1933 was an English translation available, an abridgment at that. The name of the first translator-editor, Edgar T. S. Dugdale, did not appear in the Hurst and Blackett edition, at his request; Houghton Mifflin did include it in the American version. Charges that the original had been so toned down as to constitute "whitewash" or "expurgation" or "bowdlerization" appeared quickly. The *Times* of London had run several excerpts late that summer. Chaim Weizmann, among others, complained that Hitler was made to appear as "perhaps rather radical, but on the whole [a] by no means unreasonable political thinker and agitator" (p. 9). In communicating with the Foreign Office, he added an extensive sheaf of translated ex-

cerpts omitted by Dugdale. It was Blanche Dugdale, niece of A. J. Balfour and a speaker on behalf of Zionism, who first urged her husband to undertake the task, when he had finished his multi-volume abridgment of German diplomatic documents based on *Die Grosse Politik*. R. C. K. Ensor maintained that the missing materials "really give most of the colour to the original book and are indispensable to a proper understanding of the Nazi mind" (pp. 8-9). Most succinct of the critical comment was that of President Roosevelt, inside the complimentary copy sent him: "This translation is so expurgated as to give a wholly false view of what Hitler is and says—the German original would make a different story" (p. 49). Hurst and Blackett had turned the manuscript over to a Nazi emissary in London; the extent of that government's revisions is unknown.

In this account the British Foreign Office comes off very well in its reaction to *Mein Kampf*. The authors find A. L. Rowse's charge that Chamberlain was ignorant of the book, in German or English, unfairly harsh. Diplomatic staff familiarized themselves early on—Sir Horace Rumbold's summary from Berlin was especially impressive—and they worked quietly and helpfully with such public-spirited citizens as Katherine Ramsay, Duchess of Athlone.

By early 1939 two competing full translations had been published in the U.S., another in Britain, this the work of James Murphy, whose experiences in connection with the manuscript and the Nazi government that once employed him would make a fine spy-detective thriller. There were protests, on grounds of publicizing a nightmare regime, but these were overcome by arrangements that included publishers working with respected scholars, one group headed by Alvin Johnson of the New School, and royalty profits going to refugee committees.

Numerous international legal problems are explored perhaps too fully, but the authors surely are not to be faulted for adhering scrupulously to their title and subtitle. Perhaps more might have been written of the plight of an honest scholar, Edgar Dugdale, confronted with a work much of which seemed, at the time, so bizarre as to be beneath serious consideration. He and much of his world eventually learned that this was an error.

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EDITH KURZWEIL. *The Age of Structuralism: Lévi-Strauss to Foucault*. New York: Columbia University Press. 1980. Pp. xi, 256. Cloth \$20.00, paper \$5.95.

It is interesting to learn on page one of Edith Kurzweil's book that Claude Lévi-Strauss failed to make tenure at an American university—a bitter con-

solation, no doubt, to some recent victims of our bureaucratic academic system. But while in the United States he met the linguistic structuralist Roman Jakobson, a refugee from Marxist intolerance, and this proved fruitful although—perhaps fatally to structuralist determinism—quite accidental. Kurzweil regards Lévi-Strauss as the “father of structuralism,” but surely the linguists were there first, and the brilliant French-Jewish anthropologist merely transported their insights into human social relations. He also made structuralism fashionable by the charms of his style, a gift denied to many of these mathematical model makers. Ideologically it was a “cool” challenge to the reign of Sartre’s existentialism. This occurred two decades ago. Always eager consumers of yesterday’s ideas, historians have recently taken up structuralist concepts.

Structuralism was militantly antihistorical, seeking “an exorcism of history” in favor of analysis of basic patterns that do not change but only combine and recombine themselves. But the dialectic between structure and conjuncture, pattern and event, came to be an interest of what claimed to be a new kind of history associated especially with the French *Annales* school. There was an interaction, too, with neo-Marxism, likewise with the curious passion for statistics so strong among historians emerging from graduate schools a few years ago. Structuralism induced a self-consciousness about “models,” expected to be more precise and logical than old-fashioned hunches or hypotheses. It has appealed to many seeking an alternative to both traditional “event” history and dogmatic Marxism. (See, for example, the interesting discussion of “Structural History and Yugoslav Marxism” by Michael Petrovich in *Slavic Review*, 37 [1980]: pp. 292–96.)

Kurzweil introduces the reader to the main figures of structuralist theory—Lévi-Strauss, Foucault, Althusser (lately so curiously homicidal), Lacan, the late Roland Barthes—and to some of their critics or revisers—Ricoeur, Lefebvre, Touraine, Derrida. She goes through them in a thorough manner; at times her effort to be concise results in an excessively abstract intellectual shorthand, but she also can sum up brilliantly. The book is judicious, scholarly, and accurate, although for my taste too little critical of what may often be seen as pretentious fraud or dazzling deceit on the part of these French hyper-intellectuals whose “fantastic intellectual virtuosity” the author much admires. Much of this is ideology, not science. Busy historians anxious to keep up with yesterday’s ideas because they are relevant to today’s historical research—likewise, of course, to other scholarly areas such as literature and to interdisciplinary perspectives—should make use of this intelligent digest of a huge body of con-

temporary thought; it is the best one around.

ROLAND STROMBERG
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PETER KRIEDE. *Spätfeudalismus und Handelskapital: Grundlinien der europäischen Wirtschaftsgeschichte vom 16. bis zum Ausgang des 18. Jahrhunderts*. (Kleine Vandenhoeck-Reihe, number 1,459.) Göttingen: Vandenhoeck und Ruprecht. 1980. Pp. 223. DM 19.80.

The contribution of this book can be seen at two levels. The first has considerable value in condensing three centuries of economic and social change into a pocket-sized text of some two hundred pages. The references are up-to-date and well informed; the explanations succinct and sparingly written, aided by graphs (small but readable) and tables. The layout is copy-book: introduction; three sections each dealing in turn with population, agriculture, industry/trade, socioeconomic change; and conclusion.

The second level will no doubt merit more contention. There is a contra-account of model building to explain the transition to industrialization, and this is clearly the major theme of the study. The base for 1500 begins by focusing production in the hands of the nuclear peasant family, labor-intensive and dependent on land. The apparently pervasive unity of this system nevertheless contained a fundamental contradiction. The existing social hierarchy led to seigneurs, often at odds with the motives of peasants but linked to them in the nexus of exchange payments. In consequence, the cobweb of social relationships was fragile, responding to major pressures from population growth and changes in the value system: rents, output, productivity. Expansion meant falling marginal returns and enlarged competence of the state.

With this model in mind, the book concentrates on the changes over time and attempts to group the sequence of economic and social change into three chronological sections with labels of varying vintage. The first, the “Age of the Price Revolution,” is naturally one of expansion and progressively widening socioeconomic contradictions. The second, the “Crisis of the Seventeenth Century,” signifies contraction, marked by dearths and growing state levies. The last, the “Upswing of the Eighteenth Century,” enjoyed renewed growth leading to industrialization. Ultimately the growth of populations and the creation of overseas settlements provided incentives to widen the market and shift from collectivities to individual organizations. The pressures in the feudal system, however, were selective; in some areas they promoted *Grundherrschaft*: peasant capitalism in Holland, aristocratic estates in Brit-

ain. Elsewhere, the reverse was the case, notably in Prussia and in Eastern Europe.

Inevitably the size of the book leaves much unsaid. One shortcoming—this Peter Kriedte acknowledges in the introduction—is the significant absence of the human, individual aspects of change; it is especially noticeable when entrepreneurs made commercial speculation an on-going success. Another, perhaps more serious, is the accent on social hierarchies at the expense of the spatial dimension of Europe, with those emerging foci of growth and their points of control, however imperfect. In brief, the explanations are without maps. All the same, the book gives a great deal. It also invites debate and leaves the tribune open.

FRANK SPOONER
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MICHAEL HAINES. *Fertility and Occupation: Population Patterns in Industrialization*. (Studies in Social Discontinuity.) New York: Academic Press. 1979. Pp. xiii, 275. \$18.50.

In the past five years, the new economic history has outgrown its childhood diseases and has entered a new "growth spurt." Among the subject matters it has targeted, historical demography is one of the most prominent fields. Here economists meet demographers, sociologists, and anthropologists in an interdisciplinary effort that has already led to significant progress on many interesting problems. Academic Press's series, "Studies in Social Discontinuity," ought to be congratulated for its impressive efforts in publishing many of the results of this research. Michael Haines has been an important contributor to the new demographic history, and in this book his earlier published work, together with some new material, is integrated into a quantitative analysis of marriage, fertility, mortality, and related topics in parts of the industrial world in the second half of the nineteenth century.

Fertility and Occupation is not a book for the casual and uninitiated reader of history. It is highly quantitative: about half of the book is dedicated to tables and graphs, and much of the discussion is an interpretation of the tables and a meticulous weighing of the data and all the possible biases, distortions, and errors that they may contain. Large samples are taken from the British and American censuses and a survey taken in 1889, and these masses of data are subjected to computerized analysis and adjusted, standardized, and corrected in every conceivable way. Haines displays admirable econometric ability and a familiarity with the large and highly technical literature in economic demography. The reading of the book is not always facilitated by the au-

thor's erudition: too often he relegates to references some ideas, techniques, and findings that might as well be made explicit.

What has Haines found? The most important finding is that mineworkers tended to marry younger and had higher marital fertility rates than did workers in other occupations. This conclusion is based on data from a variety of mining regions in Pennsylvania, England, Wales, and Prussia. Miners were usually well paid, but their incomes tended to stabilize at a relatively early age. Haines concludes that this led them to marry younger and have more children, so that the earnings of teen-aged children could supplement family income when the head of the family grew older. Haines also reports high infant mortality among miners (probably a consequence of high birth rates) and little evidence for fertility control before the 1880s. Mineworkers were, in some sense, unique, because they were non-agricultural workers in mostly rural areas. Haines works hard at trying to isolate the factors that accounted for their demographic behavior and draws sensible conclusions from his data banks.

Even a change in title ("Coalminer's Daughters" would be my suggestion) would not make a best seller out of this book. But it is an excellent example of that hard-nosed, laborious, sometimes tedious research required from those who do not believe that "for instance" always constitutes sufficient proof.

JOEL MOKYR
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ALBIE SACHS and JOAN HOFF WILSON. *Sexism and the Law: A Study of Male Beliefs and Legal Bias in Britain and the United States*. New York: Free Press. 1978. Pp. viii, 257. \$13.95.

This is a bifurcated work in which Joan Hoff Wilson offers several essays on the history and present status of women's rights in American law—essays of somewhat uncertain focus and very uneven depth—and Albie Sachs provides what can only be termed some chaotically organized and largely superficial material on the British legal scene, past and present. It is in no sense a systematic comparative treatment, although there is a good deal of inherently interesting and certainly important material on the problem of sexist judicial thought and legal norms.

Albie Sachs's contributions to the book—on myths in British judicial thought about women's characteristics and proper status, on family law, and on women in the legal profession—are marked by more than a fair share of banalities and an excess of preaching. His historical section of some sixty

pages deals mainly with the nineteenth century and is episodic and incomplete.

Wilson's contributions include a long essay on the topic, "Are Women Citizens?"—useful for its survey of the legal status of women in colonial and early nineteenth-century America, its review of Susan B. Anthony's challenge to and legal assaults by others on sexist interpretations of constitutional rights, and its summary account of the movement for protective legislation. Much of the material is entirely familiar to students of social and legal history, and nearly all has been touched upon, at least, by leading legal writers such as Herma Hill Kay and Ruth Bader Ginsburg. It is highly useful, however, to have the issues of "citizenship," definition of "persons," and foundations of protective legislation (with its essentially discriminatory philosophical foundation) treated afresh and with the insight of a historian well versed in the general history within which this specialized subject falls.

The essay is disappointing for its scant attention to legal history in the state courts, especially the line of decisions in several states that forms the essential context for modern cases that Wilson does review. A vastly important subject—the position of the criminal defendant—is very unfortunately reduced to the discussion of a single case, a modern one at that (*U.S. v. Patricia Hearst*). Other material contributed to this work by Wilson bears almost exclusively on contemporary problems in the law, with a brief *excursus* on "Portia's Plight," namely women's problems in obtaining access to professional careers in law. The section on current-day issues compares unfavorably in depth and contextual richness with Wilson's historical essay. Overall, this seems another of that curious but fecund species—books that ought to have been journal articles.

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ARTHUR MARWICK. *Class: Image and Reality in Britain, France, and the USA since 1930*. New York: Oxford University Press. 1980. Pp. 416. \$19.95.

The premise is intriguing. Provoked by a BBC viewer's letter urging him to clean up his slipshod appearance even if, alas, he could do nothing about his working-class background and accent, Arthur Marwick—whose middle-class Scottish credentials are impeccable and who was educated at Balliol College, Oxford—set out to study perceptions of class in Great Britain, France, and the United States. The task was herculean and, worse, not easily defined. Never one to be daunted by a large assignment, Marwick cleverly whittled this one down to size by deciding to investigate the images and realities of class as they presented themselves first in

the 1930s and then as they were transformed (to the extent that they were transformed) in the years following the Second World War. Thus we have a section on class as it was and another on class as it became.

Since the statistical realities of inequality in these three countries are relatively well established, Marwick wisely places his emphasis on images of class. It turns out that what people thought and said about class depended both on their position in society and the nation to which they belonged. Privileged Englishmen went to lengths to conceal its existence and considered it bad form to talk about the question. Privately, most no doubt felt like Duff Cooper who wrote in 1953 that a classless society "would be as useless as a rankless army and as dull as a wine-list that gave neither the name of the vineyard nor the date of the vintage." French intellectuals fancied themselves as *travailleurs* and commonly divided society into the bourgeoisie and the people. Said Edmond Goblot, a provincial philosophy professor who wrote a famous book on French class distinctions: one could never confuse a "monsieur" with a "man," nor a "lady" with a "woman." Americans of all social strata were reluctant to identify themselves with a class. Robert Redford's recent film *Ordinary People* is about a tax lawyer and his family living in upper-middle-class comfort in Lake Forrest, Illinois.

From these images and the realities that underly them, Marwick draws three general conclusions. The first is that classes exist and are not mere concepts useful for thinking about society. The second is that there has been, and still is, in these three countries an upper class that enjoys (in Richard Crossman's words) "a facility of freedom and an amplitude of life" denied to those below them. Finally, Marwick argues that the nature of class has changed between 1930 and the present. The composition of the three strata into which society is divided has been transformed, while there has been a tendency for the social structures of the United States and the two European countries to converge. Class feeling, he says, has strengthened in the United States, while Britain and France have become more mobile and affluent societies, troubled by problems of race and ethnicity formerly thought typical of the United States.

It is hard to know how seriously to take these propositions because it is hard to know how seriously Marwick himself takes them. Like many historians, he likes to poke fun at sociologists and their concepts and takes a playful delight in the messiness and intractability of history and life. Yet he is also fond of telling us that classes exist even when they remain unperceived. As he characteristically puts it: "because the spectator at the back of the gods cannot see the chorus girl's cleavage, that does not mean that it does not exist." Fair enough. But what

is the significance of this cleavage if it goes unperceived? Or, to use an analogy more in keeping with the findings of his book, what is the significance of this cleavage if it is perceived by the man in the gods in one way and by the man in the stalls in another? Marwick himself seems undecided, for in summarizing the impact of class on the events of the 1930s he tells us that whereas these events cannot be explained solely by reference to class, neither can they be fully understood without reference to class.

At this juncture many readers may feel that a mountain of research is about to give birth to a molehill of explanation. But the reader who perseveres will gain some stimulating insights into the parallel and divergent development of these three countries and will receive a salutary lesson in the baffling variety of ways in which Britons, French, and Americans have perceived and talked about class divisions during the past fifty years.

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GEOFFREY BEST. *Humanity in Warfare*. New York: Columbia University Press. 1980. Pp. xi, 400. \$25.00.

The history of international law is not precisely an overpopulated field—viewed by diplomatic historians as marginal to the flow of events and generally disdained by international lawyers whose intellectual style runs to doctrinal formalism. Consequently, any historical excursion into the development of international legal rules is an occasion for curiosity, and in this case for much else besides. For Geoffrey Best has provided a model of how such scholarship ought to be undertaken and why it is worth doing.

Humanity in Warfare appears on cursory examination to be simply a narrative of the attempts from the late eighteenth century to the 1970s to mitigate the sufferings of those touched by organized combat, including combatants, civilians, prisoners of war, guerrillas, and neutrals. The great monuments of these efforts were, of course, the Hague Regulations for Land Warfare, the Geneva Conventions, and their legal progeny. Best has, to be sure, painstakingly followed the legal trail, but he has done much more besides. The great virtue of the work lies in the amplitude with which the topic has been defined. Rather than viewing the task as one of simply tracing the appropriate documents across the decades, the author loses no opportunity to establish the context within which legal maneuvers took place. In particular, the work is filled with trenchant observations on the relationships between military and civilian policymakers; the economic interests engaged by warfare at sea; and the subtle, often insidious, manner by which acquiescence in

small changes in military practice became precedents used to justify their subsequent expansion. If the work has a single great "set piece," it is the account of the development of strategic bombing in the Second World War on the road to Dresden and Hiroshima.

No work, even as rich as this, is without its faults. In this case, there is a regrettable tendency to view technological factors as only peripherally significant, an omission not repaired by a few obligatory pages here and there on the submarine and atomic weapons. Perhaps because of a slight bias toward things English, perhaps as well because the account of the post-1945 period is concededly fragmentary, there is scarcely a mention of Vietnam, although the book is filled with implications for it. Finally, Best's facile style can be an instrument for eloquence and passion but also unfortunately for wordiness and an intermittent "chatty" informality that seems out of keeping with the subject. In the balance, these are small failings indeed for what will surely stand as a landmark work.

MICHAEL BARKUN
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ANCIENT

PHYLLIS YOUNG FORSYTH. *Atlantis: The Making of Myth*. Montreal: McGill-Queen's University Press or Croom Helm, London. 1980. Pp. x, 209. \$19.50.

The twin themes of discovery and destruction on an epic scale are obsessions of the contemporary consciousness. In recent years there has been exciting exploration of the moon and outer space, coinciding with the publication of several books concerned with the destruction of the mythical civilization of Atlantis.

J. V. Luce (*The End of Atlantis*), James W. Mavor (*Voyage to Atlantis*), and A. G. Galanopoulos and Edward Bacon (*Atlantis: The Truth Behind the Legend*) were preoccupied with the possible connections between three major topics. These are, firstly, the violent destruction of the Minoan civilization of Bronze Age Crete; secondly, an equally violent eruption of the volcanic island of Santorini (ancient Thera); and, thirdly, the subsequent chance of discovering new reality behind that legend of Atlantis that has haunted the imagination of writers from antiquity to the present day.

Contemporary authors have approached their subject from differing points of view, but they have all been stimulated by the discoveries made on Santorini under the late Professor Marinatos since 1967. The suggestion that Atlantis reflected Minoan Crete was not made and could not have been made until the archaeological discoveries of this century revealed once more the achievements of Minoan art and technology. Although Santorini (on present

evidence) might well have been explosively destroyed about 1500 B.C., there is still a puzzling time-lag between this destruction and the collapse of the principal centers of Minoan civilization perhaps half a century later. In the book under review, Minoan Crete is given place as a factor in the Atlantis legend but not as an essential factor in the development of the legend: "Even if Minoan Crete was the main model for Atlantis, it was nevertheless a *limited* model. Plato clearly supplemented features derived from Crete with features from other sources."

What is so refreshing is that Phyllis Young Forsyth not only begins at the beginning but goes on to the end with some enticingly new suggestions. She writes persuasively in her ordered discussion of the enigma of Atlantis; of Plato and the cast of characters; of the historical aspects of Greek myth; of Plato and myth; of the Atlantis location; of alternative sites for Atlantis; of volcanoes and Thera; of the excavations on Thera; of Minoan Crete and Atlantis; of the Platonic synthesis; and of the lure of Atlantis.

Apart from its pleasing style and thoroughly reasoned historical approach, the arresting originality of this book lies in its conclusion that Sicily is plausibly a real location for Atlantis, with (of course) a background of orally transmitted accretion. Plato's writings and Plato's experiences in Sicily are accorded proper consideration within the environment of the growing legend. We are brought from the worthy but speculative endeavors of archaeologists and prehistorians to the perhaps equally speculative testimonies of Plato in the *Timaeus* and the *Critias*. The case presented by Phyllis Young Forsyth commands respect and serious consideration, not least because she is sensitively aware that Plato, in his life and in his writings, was so much nearer to his past than we are.

R. F. WILLETTS
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FRANK J. FROST. *Plutarch's Themistocles: A Historical Commentary*. Princeton: Princeton University Press. 1980. Pp. xiii, 252. \$17.50.

In two preliminary chapters, Frank J. Frost provides essential background. The first skillfully traces the depiction of "Themistocles in the Literary Tradition" from the fifth century B.C. to Hellenistic times. Included are detailed analyses of content in authors such as Herodotus, Thucydides, and Ephorus, along with a thematic scrutiny of "The Philosophers" and "The Political Theorists." Missing, in this context, is some overall consideration of the letters attributed to Themistocles, a substantial corpus of dramatic material, which, for better or worse, cannot be ignored. They represent, not unlike Plutarch himself, a late crystallization of both fac-

tual and fictional Themistocleana. Frost does, on occasion, consult these letters critically, not all which references, by the way, are to be found in the sometimes inadequate index; yet he might have put them to better use in his discussion, say, of the ostracism and exile (pp. 192-99).

The second preliminary chapter, "Plutarch, Biography, and History," offers an informative appreciation of method and style in the *Lives*, with particular argument for Plutarch's first-hand knowledge of his sources. All the more pity, then, that no attempt is made to focus this generic view in terms of the *Life of Themistocles*.

The commentary itself, which, as the title indicates, is primarily historical, divides Plutarch's chapters into major sections entitled: "Themistocles' Pre-War Career," "The Invasion of Xerxes," "The Battle of Salamis," "Triumph and Decline," and "Flight and Exile." Within the framework of textual elucidation, more extensive excursions on the archon year, 493-92, the Salamis campaign and Themistocles' downfall provide meaningful enrichment; we are even rewarded by a not-so-historical sketch of the dramatic action and romantic themes in Plutarch's treatment of Themistocles at the court of the great king.

All in all, Frost's interpretations are reasonable, challenging the more bizarre in Themistoclean scholarship; he keeps Themistocles' archonship in 493-92 and, among his conclusions, those concerning chronology represent, as well as anything, his sane caution—a verdict that I hope means something other than the mere fact that, more often than not, I agree with him. Yet, fortunately indeed for the vigor of our discipline, everyone cannot view all the evidence in quite the same way. In particular, I should question the dubious insistence upon the complete lack of individual political tendencies in the early fifth century (pp. 29-31) and the unequivocal acceptance of the thesis (recently and convincingly challenged by D. H. Kelly) that the archonship was not the highest office in Athens in the 490s (pp. 74-75). Frost's *Commentary*, nevertheless, stands as an essential work of reference for every classicist, because of its serious scholarship, reflective judgment, and, not least of all, its clarity of presentation.

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CHRISTIAN MEIER. *Die Entstehung des Politischen bei den Griechen*. Frankfurt a/M.: Suhrkamp. 1980. Pp. 514. DM 48.

The Greeks have contributed many things to Western civilization but perhaps in no area has their achievement been more persistent and more controversial than in politics, especially in the develop-

ment of democracy and the various institutions and practices associated with it. Christian Meier in his *Die Entstehung des Politischen bei den Griechen* is interested in exploring a total view of history and in seeking to understand the ancients within the context of a consistent theory of history. For him it is important to understand the social history that gave rise to the political thought and to study the Greek experience of politicalization not only for what it tells us about the Greeks but also for what it can do for our own political understanding and development. History for Christian Meier involves a constellation of factors that reveal interconnections in time and space, and the different forms of historical change are to be differentiated as are the relations between event and change, politics and process, the different relations between change and perceptions of change, and the different methods of knowing about human capabilities and power. The formation of "the political" (*das Politische*) is not a matter that can be clarified simply by demonstrating that the human capacity for high culture can produce exceptional political capabilities. The peculiarity of Greek culture does not help clarify the situation since it too was the result of a process in which the condition of the possibility of democracy originated. The first democracy of world history could only come into being if the political community of its citizens was at the center of their lives and that citizenry had a special place, a distinct power, a social identity in a specific period of time, a special relation to a given place and mode of life, particular common experiences, and a shared comprehension of human capacity, historical events, society, and change.

Die Entstehung des Politischen bei den Griechen consists of seven studies—all previously published and some considerably reworked for this volume. Even though these studies may occasionally overlap, they are united by three general questions: (1) How did democracy originate among the Greeks? (2) What distinguishes the politics of the ancient Greeks? (3) How can these politics be expressed in terms of world history and historical interpretation? The three general sections of the book are devoted to the notion of politics, prehistory and the origin of Greek democracy, and politics and time perception and notions of the political world of the Greeks and of change in that world. The reforms of Kleisthenes, Aeschylus's *Eumenides* as evidence of early political thought, the political identity of Athens and the achievement of Periclean thought, and Greek historiography of the fifth century (with special attention to Herodotus, the idea of progress, ancient views of the origin and nature of history, historical knowledge, and historical consciousness) are themselves discussed in considerable detail, with full familiarity with classical and contemporary historical scholarship.

Die Entstehung des Politischen bei den Griechen helps us understand the ancient Greeks better, but it also serves to raise certain basic questions in which all historians are interested.

JOHN E. REXINE
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JOHN F. HEALY. *Mining and Metallurgy in the Greek and Roman World*. (Aspects of Greek and Roman Life.) London: Thames and Hudson. 1978. Pp. vii, 316. \$19.95.

The innovation of analytical examinations of physical and chemical artifacts from past cultures has revolutionized the study of many aspects of historical research. When R. J. Forbes published his multivolume *Studies in Ancient Technology* (1959–64), he had little more to start with than the scattered mentions of industrial processes in the remnants of classical literature. Since the Greco-Roman world had no technical or trade press, however, few facts about such industries ever got into written form. John F. Healy very skillfully weaves together the results of modern industrial archaeology applied to classical sites and the documentary evidence. The book's ten chapters start with the geographical background of ancient mining sites, deal in detail with the types and sources of ores, and survey the manufacture, characteristics, properties, and uses of compounds and alloys.

One of the most interesting sections in this very well-researched study is the chapter on mine administration. In this segment the author reviews all of the linguistic evidence for the terms in Greek and Latin for miners and the mine hierarchy and then reviews the information about workers and working conditions in ancient mines. Among some of the observations that the author makes are the fact that work in the mines was so dangerous from frequency of collapse and noxious fumes that ordinary slaves were considered too valuable for such work and condemned criminals were preferred for the mines. Healy cites Diodorus Siculus (*Loeb Classical Library* edition, III, 13, 1), "but to continue with the mines, the slaves who are engaged in the working of them produce for their masters revenues in sums defying belief, but they themselves wear out their bodies both by day and by night in the diggings under the earth, dying in large numbers because of the exceptional hardships they endure. For no respite or pause is granted them in their labors, but compelled beneath blows of the overseers to endure the severity of their plight, they throw away their lives in this wretched manner" (pp. 134–35).

The book is thoroughly documented with over seventy-five high-quality photographs of sites and artifacts of ancient mining and metallurgy and a forty-three page section of small-print footnotes and

citations. Such care and detail make this the most authoritative work on ancient mining to date under one cover.

One further observation must be made that in this field of somewhat narrow interest the author's excellent style, unencumbered by the usual academic debates, and the publisher's willingness to place the book in a pleasing format, have produced an interesting and readable account suitable for the dedicated scholar and the casual history buff, which in itself is no mean feat.

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M. I. FINLEY. *Ancient Slavery and Modern Ideology*. New York: Viking Press. 1980. Pp. 202. \$13.95.

In four substantial chapters based on lectures delivered at the Collège de France, Sir Moses Finley considers the central issues in the study of slavery among the ancient Greeks and Romans: the impact of modern scholarly obsessions (antiquarianism, philology, ideology) on analyses of slavery, the rise of slavery in the ancient world, the alleged influence of humanitarian instincts upon the slave system, and the gradual disappearance of slavery in late antiquity. Finley presents his arguments with exemplary lucidity and a measure of good common sense that is as bracing as it is polemical. The writings of some of the most renowned scholars of classical slavery—Eduard Meyer, W. L. Westermann, and Joseph Vogt in particular—are subjected to harsh critiques. There is even something parricidal about the assault on Westermann, with whom Finley had studied when he was a graduate student at Columbia. But it is helpful to locate Westermann's faults, which have been generally acknowledged for several decades, in the context of Meyer's once authoritative paper ("as close to nonsense as anything I can remember written by a historian of such eminence" [p. 48]) and the scholarly *ergastulum* organized by the editor of *Rom und Karthago*, who comfortably moved from *Rassengeschichte* to *Humanität*.

Finley offers no easy solutions to old problems. He shows that the Romans did not take up slavery as a result of the wars of conquest that made slaves readily available but that there was a prior demand. He demonstrates that slavery was as brutally inhumane in the days of Constantine as it was in the time of Spartacus. It is a comment on the academic capacity for self-deception that this should ever need demonstrating. The existence of individual masters who were kind as well as of individual slaves who were happy (or thought they were) is a different matter altogether. In the last chapter, on the decline of slavery, Finley effectively demolishes the familiar notion that moralists, Stoics or Chris-

tians, caused a salubrious change of attitude that brought on the death of the system; he shreds the popular hypothesis that the drying up of an external supply of slaves made slaves so expensive as to be unprofitable. Slavery died out only gradually as other sources of internal labor became available. No one could take credit for abolishing it.

Although Finley is sensitive to Marxist interpretations of ancient slavery, he does not hesitate to reject these interpretations when he sees fit. He takes issue several times with the views of Shtaer-man. While he fulminates against those who ignore Marxist views, his could not be called a Marxist account. In fact, what is remarkable about this volume is that in exposing the ideological preconceptions of others Finley has managed to avoid substituting a set of his own. Accordingly, much remains uncertain, many questions remain unanswered. This may well be Finley's best book.

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CLAUDE NICOLET. *The World of the Citizen in Republican Rome*. Translated by P. S. FALLA. Berkeley and Los Angeles: University of California Press. 1980. Pp. 435. \$50.00.

Claude Nicolet's *Le métier de citoyen dans la Rome républicaine*, published in 1976, here receives a competent and welcome English translation. The work merits a wider readership. Nicolet directs his characteristic acumen to a broad-gauged study of civic life in the Roman Republic. His approach is sociological, rather than legal or political. A comprehensive vision orders the detailed discussions of rights, activities, and obligations that attach to the citizen, thus providing both a firm line and a stimulus for argument.

Three major aspects of the citizen's role comprise the object of study: the military, the fiscal, and the political. For Nicolet, these are interlocking functions that form a seamless whole. The census, in his analysis, lay at the heart of Rome's civic organization and served to define the "integrated citizen." It graded Romans into property classes, equitably apportioned military burdens, financial responsibilities, and political privileges, and thereby brought about a social cohesion that endured through most of the Republic.

This study ranges over three centuries or more of Roman history. The sweep gives it considerable importance but also imposes strain on the thesis. Nicolet explores the factors making for integration yet takes in the period that, in his view, promoted disintegration. Since the work is organized topically rather than chronologically, unresolved tensions arise in the argument. So, for example, there is con-

fusion as to whether, when, and to what extent the citizen militia became a professional soldiery in the Republic. Nicolet alternately sharpens and softens that evolution (compare pp. 97, 108, 129–37). Similar difficulties surround the discussion of finance in terms of social cohesion. What was the relative impact of the *tributum* as civic responsibility, of its long-term suspension and the reliance on provincial revenues, of public controls on the tax-farming system, of the burdens and controversies generated by grain distribution? This combination of diverse issues without clear analysis of their mutual influence over time weakens the effect (compare pp. 155, 169, 172, 188, 201, 204). And, with regard to political activities, did opportunities expand or contract for the average citizen, did electoral bribery pervert the system or touch it only at its margins, did growth of the civil service restrict or promote *libertas*, did the proliferation of statutory law advance justice or render it more problematical? (compare pp. 267–70, 286–89, 298, 306, 311, 324–34, 340–41). On all these issues Nicolet has illuminating remarks, but the loose ends and unanswered questions prevent his argument from running a smooth course.

A more fundamental tension pervades the book. The bulk of it consists of a fresh and penetrating analysis of institutional structures: the operations of the census, military organization, fiscal administration, and the forms and practices of political participation. Then Nicolet shifts attention to extra-institutional features of civic behavior, the growth of practices that expressed public sentiments while remaining outside the official system: private and semiprivate associations, ceremonies like major funerals and triumphs, and demonstrations at shows and public trials. These developments add intriguing dimensions to the picture. But how far they paralleled traditional institutions and how far they supplanted or eclipsed them remains obscure in Nicolet's presentation.

The difficulties in this work also supply testimony to its richness and scope. Nicolet stimulates thinking on almost every page. And he has given a vivid reconstruction of the civic life of the average Roman—not a mere instrument of politicians but actively engaged in the institutional structure while gradually developing modes of expression as an alternative to that structure.

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RUDOLF PESCH. *Simon-Petrus: Geschichte und geschichtliche Bedeutung des ersten Jüngers Jesu Christi*. (Päpste und Papsttum, number 15.) Stuttgart: Anton Hiersemann. 1980. Pp. vi, 193. DM 120.

The present volume is one in a series called *Päpste und Papsttum*. This series is a significant new contribution of German scholars to historical research and will, no doubt, be used as a reference by future generations. The project, which is not expected to be completed for many decades, will include biographies of popes and monographs on various aspects of the papacy. The editors expect that eventually the series will become the first comprehensive work on all the popes and the entire institution of the papacy. Up to this time seventeen volumes have been published, thirty-three more are in preparation, and many others are planned. Clearly this is a major undertaking. According to the editors it will not be sectarian in nature; scholarly qualifications alone will be the criteria in the selection of cooperating authors.

Yet the inclusion of Simon Peter in the list of popes immediately betrays a certain bias, or at least a preconception that Peter was indeed the first of the popes and the beginner of an "apostolic succession" of Roman bishops. This is a debatable thesis. As a matter of fact, in spite of the perhaps hasty announcement by Pope Pius XII on December 23, 1950, that the grave of the "prince of the apostles" had been discovered under the main altar of St. Peter's basilica in Rome, there are no certain historical facts concerning Peter's activities and martyrdom in Rome. The question has even been raised whether he ever was in Rome at all. Among the first who expressed doubts was Marsilius of Padua in his *Defensor Pacis* (1326). More recently a substantial literature has arisen on the subject, including H. Lietzmann's *Petrus und Paulus in Rome* (1915; 1927), which decides in favor of the two apostles' deaths in Rome. Lietzmann is violently opposed by K. Heussi's, *Was Petrus in Rome?* (1936), but the question has been more or less decided in favor of Lietzmann's thesis by his famous student, Oscar Cullmann in his *Petrus Jünger-Apostel-Märtyrer* (1952; second revised edition 1960). Of course the literature on the subject is immense and the interested reader will find a nearly complete bibliography, including works on the excavations under St. Peter's, at the end of Pesch's book.

In addition to the elusive archaeological evidence, what other sources are available for the historian in his quest for the story of Peter? Strictly historical sources are the gospels only—provided that one would accept them as objective history and not as reflections of a later, believing Christian community on the events that happened a generation or two before. The later apocryphal literature contains only legends and after that, of course, tradition sets in. Occasional glimpses of Peter can be gained from other books of the New Testament, especially Acts and the letters of Paul. It is no wonder, therefore, that Pesch's book, which is supposed to be a biogra-

phy of Peter, is mostly exegesis and interpretation. In these tasks he shows himself in full control over his material, handling it with impeccable scholarship and academic integrity. Nevertheless, this is a book that will be more useful for theological experts, especially those specializing in New Testament studies, than for historians.

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PAUL MACKENDRICK. *The North African Stones Speak*. Chapel Hill: University of North Carolina Press. 1980. Pp. xxi, 434. \$21.00.

Skillfully blending a small amount of political and military history with extensive archaeological material to produce a running narrative of North African civilization from prehistoric times to the Arabic invasion of the seventh century A.D., Paul MacKendrick attempts to do for ancient North Africa what he has previously done for Greece, Spain, France, and other areas once occupied by the Romans. Our knowledge of ancient Africa, except for Egypt and the southern reaches of the Nile, is largely confined to the rather thin coastal area from modern Morocco to the Cyrenaica. The interior was largely unknown. Extensive remains from the Greek and Roman periods permit one to form a fairly good picture of the peoples and the cities. In the first part of this book MacKendrick surveys Tunisia, describing first in some detail through the historian's eyes the physical culture of the Carthaginians down to their destruction by the Romans, followed by the new order of things under Roman administration to the arrival of the Arabs. In the second part he does the same for Libya, then Algeria, and finally Morocco (the ancient Mauritania). As usual, he has prepared himself well for this volume, marshaling a vast amount of archaeological data to give us rich panoramas of what life must have been like in the ancient cities of this area, including detailed studies of the cities themselves, their buildings, monuments, artifacts, and the extant traces of Roman border fortifications. At times one can see and feel the people who once lived there. Not designed for scholars, nevertheless this book will be most useful to them for the careful synthesis of information about the various sites and the rather full bibliography for each of them at the end of the volume. There are excellent photographs and plans but, as usual, mediocre maps. Most annoying is the lack of exact references in the body of the book to the sources that support the author's statements. Very frequently, he will explain or describe fascinating artifacts or discoveries without telling us where we can find the full report or a fuller descrip-

tion. Short notes could have made the book much more useful and interesting.

Despite a romantic impulse within the author to introduce allusions to modern literature like Flaubert's *Salammbô* or modern painting like John Vanderlyn's *Marius Amid the Ruins of Carthage*, this book can be recommended to ancient historians, not for the very meager amount of political or military history it contains but for the full and rich descriptions of the archaeological sites. Especially useful is MacKendrick's final chapter called "The Balance Sheet of Empire," in which he offers us a series of succinct observations on Rome's contributions to North African civilization, although he cannot resist the temptation to allow Roman "colonialism" to lead to the subject of modern colonial developments in the land. He even suggests that the modern movements toward political independence "may well be the fruit of a seed first sown by ancient Rome." Here MacKendrick is out of his element. And some historians may be irritated occasionally by his style, which sometimes leads to an excessive compression of events and odd chronology: "Sixty years ago, Gigthis was an exotic spot: the excavators reported flamingoes on the beach. Its prosperity came from agriculture; it sent grain direct to Rome's port of Ostia" (pp. 54-55). There are other examples of such time travel. But it would be picayune to dwell on these matters and neglect the stated purpose of the author to use archaeology to write cultural history. This is difficult to do well. He has succeeded better in some of his other works. This one is very readable and interesting.

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MEDIEVAL

ERNST-DIETER HEHL. *Kirche und Krieg im 12. Jahrhundert: Studien zu kanonischem Recht und politischer Wirklichkeit*. (Monographien zur Geschichte des Mittelalters, number 19.) Stuttgart: Anton Hiersemann. 1980. Pp. ix, 310. DM 160.

Ernst-Dieter Hehl's book, a revision of his 1977 Mainz doctoral dissertation, deals with the difficult problem of the twelfth-century church's views on violence. The evidence that he explores consists mainly of canonistic writings from the second half of the twelfth century, that is, the works of Gratian and the early decretists from Paucapalea to Johannes Teutonicus. Part of this ground has been tilled by earlier writers, in particular Carl Erdmann (1935), Alfons M. Stickler (1942-54), Frederick H. Russell (1975), Jonathan Riley-Smith (1979), and

this reviewer (1969, 1976). What, then, is new about Hehl's approach?

In part, Hehl has edited a few manuscripts that his predecessors had not used; he furnishes these texts, with notes on variants, in a short appendix (pp. 262–67). Most of the manuscripts drawn upon are in the Staatsbibliothek in Bamberg. The sources themselves are not obscure—Sicard of Cremona, Huguccio, and the *Summa Elegantius in iure diuino*—nor are the variants critical, but this remains useful work. Also useful, and more novel, is the apparatus of canonistic texts that Hehl supplies as sources for the sermon preached by the bishop of Porto to the participants in the second crusade. While the text of the bishop's interesting discourse has previously been edited, earlier editors were not aware of the extent to which the sermon was based upon canon law.

Finally, the author attempts to relate the canonistic theories about violence and warfare to the political practice of the twelfth century, notably regarding the electoral disputes involving King Lothar III of Germany and Pope Innocent II, the appearance of the Templars and other military orders, and the second crusade. His attempts to define that relationship are sometimes inconclusive, but his examination of the problem makes provocative reading. He is almost certainly correct in his basic thesis that the law of war as it appears in Gratian and the decretists was grounded in reality and was not merely a theoretical construct.

JAMES A. BRUNDAGE
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SIMON KEYNES. *The Diplomas of King Æthelred "The Unready," 978–1016: A Study in Their Use as Historical Evidence.* (Cambridge Studies in Medieval Life and Thought, third series, number 13.) New York: Cambridge University Press. 1980. Pp. xix, 295. \$42.50.

The British Academy is sponsoring a definitive edition of all Anglo-Saxon documentary records. This is a cooperative venture, each editor being assigned responsibility for an archive or a group of archives. Two volumes have appeared. The decision to proceed by archive rather than chronologically is understandable and defensible, but it does postpone any reconsideration of individual reigns or of the period as a whole until the project is complete. Simon Keynes has, so to speak, jumped the gun with this study, based on a collective examination of all the royal diplomas of King Æthelred.

Keynes has very little material to work with. For the thirty-nine years of Æthelred's reign there are 113 royal diplomas extant: 83 apparently authentic,

11 of questionable authenticity, and 19 spurious ones. There are none at all for four years of the reign, only spurious ones for another four years, and one only for each of another eight years. In no single year does the number of authentic diplomas exceed six. Little wonder that this book is saturated with "could," "would," and "might."

The diplomas "have an important contribution to make to the administrative and institutional history of the reign" (p. xvii). Their contribution, after 150 pages of painfully meticulous analysis, turns out to be "the existence, during the reign of King Æthelred, of a royal secretariat responsible for the production of administrative communications and other documents associated with the exercise of government, and there is also evidence that Æthelred had a seal and that his archives were kept with the royal collection of relics" (p. 152). But we are told in a following footnote that there is no conclusive evidence that the presumed royal scribes were organized into a department. This leaves us virtually back at Pierre Chaplais's assertion in 1973 (quoted on page 22) that "there is not a shred of evidence to indicate that at any time between the seventh and eleventh centuries Anglo-Saxon diplomas were drafted or written in what might be called, even loosely, a central royal secretariat."

Since diplomas created bookland, consent of the witan was required, and therefore witnesses to diplomas would have been in attendance. A prosopological analysis of the witness lists leads Keynes to suggest a new framework for reconsideration of the reign and of the traditional judgment on the king. The course of Æthelred's life was beyond his control during the period of tutelage (978–84), filled with youthful indiscretions from 984 to 993 owing to the influence of unscrupulous men, well conducted during the years of maturity (993–1006), and certainly out of hand during the closing decade (1006–16).

What this learned work—the figures, tables, and *apparatus criticus* are overwhelming—teaches us is, to paraphrase Sir Maurice Powicke, that the historian knows enough to know that he cannot know.

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ZVI RAZI. *Life, Marriage and Death in a Medieval Parish: Economy, Society and Demography in Halesowen, 1270–1400.* (Past and Present Publications.) New York: Cambridge University Press. 1980. Pp. xi, 162. \$27.50.

Zvi Razi makes use of an exceptionally good series of court rolls to study Halesowen manor, in the English West Midlands, from 1270 to 1400. Razi claims that most interpretations of medieval country life are flawed because they are not based on ex-

tensive local research. He remedies this; the book's greatest scholarly contribution is its presentation of some of the best demographic data available for fourteenth-century England.

Razi argues for population growth from 1270 until the great famines of the 1310s, a population setback of perhaps 15 percent in the 1310s, and renewed growth from the 1320s, culminating in a peak in the 1340s—trends that run counter to those suggested by M. M. Postan. The author rejects G. C. Homans's theory of late age of first marriage and accepts instead J. Hajnal's idea of early nuptial patterns before 1500. Razi follows R. H. Hilton in claiming that preplague conditions allowed the rich to grow richer while the poor got poorer, arguing for deteriorating social and economic circumstances for the bulk of the populace. Between 1270 and 1348 demographic differences in Halesowen were created by economic difficulties, and not vice versa.

The Black Death changed everything. It killed close to 50 percent of the manorial population and created a more fluid and open society. A rise in life expectancy and improvements in living standards resulted. Marriage took place often and at early ages. Illegitimacy declined and there was a rise in the number of childless families, the result, according to Razi, of higher age-specific mortality among the young. The traditional medieval seigneurial economy declined, and customhold tenure gave way to copyhold.

The book has many strong points. The introduction sets the geographical stage quite nicely and tells a good deal about local economy, society, and peasant obligations. Chapter 1, on method, is clear, concise, and easy to follow. Major problems, such as identification of individuals with similar names and linkage of families through time, are fully discussed. A fine method of estimating peasant wealth is presented, based on a combination of landholding size, village offices, and family size. In all, the argument is well reasoned and presented.

The book also has a number of shortcomings. At least one is beyond the author's control; Halesowen manor consisted of a collection of small hamlets rather than a nucleated village. This raises questions about any general application of Razi's conclusions. At certain stages, his data base is very small, sometimes containing less than ten cases *per annum*. The data also have a pronounced male gender imbalance. His secondary book citations are sometimes outdated; perhaps this is why he erroneously calls the epidemic of 1375 "the century's fourth and last outbreak of plague" (p. 128). Most seriously, the book has weak and rather speculative conclusions that negate many of its earlier strengths. Razi argues that the plague changed the age structure of late fourteenth-century population primarily because of its harsh effects on children.

This in turn led to an increasingly aged population, falling fertility, and demographic decline. Razi does not present sufficient evidence to support these assertions.

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G. W. S. BARROW. *The Anglo-Norman Era in Scottish History*. New York: Clarendon Press of Oxford University Press. 1980. Pp. xxiii, 232. \$49.50.

In addition to its avowed purpose "to describe and evaluate the Anglo-Continental contribution to the medieval *regnum Scotie*" (p. 156), this book has the implicit secondary objective of breaking down what G. W. S. Barrow calls "a curiously malign tradition of insularity among English and Scottish historians" (p. 3). Although the book fulfills its primary purpose, it remains unclear how this research can be incorporated in any significant way into the writing of English history for the period from 1097 to 1296. Even the specific comparative discussion of the Scottish and English armies will not produce the substantial change in the debate over the origins of English feudalism that the author seems to expect. The Scottish evidence shows the similarity of a pre-1066 territorially based army in England and Scotland and the survival of a territorial army in Scotland alongside feudal knight service when it was introduced there later. This evidence does throw some light on the English experience by contrast with Scotland, but it is hardly clear enough to add much or to terminate this long-standing debate.

The main contribution is Barrow's thorough examination of the pattern of migration and colonization during the twelfth and thirteenth centuries when settlers came into lowland Scotland from England and from France and Flanders. He shows that the Anglo-Norman settlement in that period reinforced an earlier Anglo-Saxon settlement to produce an area in Scotland where English was the predominant language and a settlement pattern that had a decisive effect on the texture of Scottish society as a whole. Both the difficulty and the measure of the author's successful achievement are indicated by the types of sources available, in which he works with evidence "scattered . . . among charters and chronicles, in surviving and obsolete place-names, in remains or traces still to be seen on the ground of motte castles, village sites, parish kirks, and dependent chapels" (p. 33). In fact, the book could be read as a case study of how to deal with such intractable source materials to obtain results not only for a handful of aristocratic families but also for migrants lower on the social scale. A

lengthy appendix on thirty-five families will have specific value for reference purposes.

Although given as the Ford Lectures at Oxford in 1977, there are touches of Scottish nationalism that seem out of keeping with a scholarly monograph. An example of this style is: "When royal ladies were married furth of Scotland in the thirteenth century their tochers (marriage portions) tended to take the form of cash (p. 63).

This book gives a good picture of the amalgamation that produced a distinct frontier society on the north side of the border between England and Scotland and a demonstration of how an experienced historian can use even seemingly inadequate sources to produce sound results. Within its limits the book is successful, but the results will not convince the historian of medieval England of the necessity to look north of the border for help in solving problems of interpretation.

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MARGARET WADE LABARGE. *Gascony, England's First Colony, 1204-1453*. London: Hamish Hamilton; distributed by Barnes and Noble, Totowa, N.J. 1980. Pp. xii, 276. \$23.50.

In *Gascony, England's First Colony, 1204-1453*, Margaret Wade Labarge surveys a most important aspect of Anglo-French medieval history. Over the past few decades, scholarly interest in both the origins of the Hundred Years' War and the evolution of the English and French monarchies has led to a considerable amount of archival research in the history of English Gascony and the publication of numerous primary sources and monographs. The latter particularly, explaining in detail English administration of Gascony as well as Anglo-French diplomacy and conflicts over the duchy, have rendered earlier summaries inadequate.

With these great contributions of scholarship at her disposal, Labarge seeks to summarize anew England's first experiment in the governance of an overseas possession. Although her survey is along traditional chronological lines, she enlivens the narrative with descriptions of persons such as Simon de Montfort and Pey Berland who played critical roles in English Gascon history. She also stresses throughout factors such as the ducal government's financial instability, which she believes to have been the source of constant difficulties during the long period of English rule. Yet, she concludes, the English provided their duchy with relatively good government, at least until the last fifty years of rule.

Although she emphasizes the great importance of

Gascony in the origins of the Hundred Years' War, the author does not attempt to give a total account of that conflict. Noting that Gascony was never the focal point of military action and that the English increasingly neglected the duchy in the later stages of the war, she does provide detailed explanations of the campaigns in Gascony and describes their often important results. In summarizing, Labarge states that both England and Gascony were significantly affected by their long-term relationship and believes that the experience "helped to lay the foundations for the far-flung empire based on sea-power which England was to develop over the next three centuries" (p. 237).

Medievalists can take delight that such an up-to-date summary of English Gascon history, both indexed and illustrated, is now available. Yet their joy must be tempered with regret that the author sometimes neglected important sources and made careless stylistic errors. Omitted from her notes and bibliography are such standard primary and secondary works as the *Actes du Parlement de Paris* and T. S. R. Boase's *Boniface VIII*. Had she used the *Actes*, for example, her description of the Gascon appeals to French jurisdiction would have been much better informed. Furthermore, Boase's work would have provided her with an explanation of why the pope arbitrated as a private person between the English and French kings. Finally, greater care in writing would have helped her avoid such unfortunate sentences as "The peasants fled to the towns, for safety behind the walls and because they could no longer make a living from their devastated lands" (p. 205).

Despite these qualifications, it is fair to say that Labarge's book will provide students and general readers alike with a useful introduction to this important part of medieval European history.

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J. R. LANDER. *Government and Community: England, 1450-1509*. (The New History of England.) Cambridge: Harvard University Press. 1980. Pp. vii, 406. \$20.00.

In the "New History of England" J. R. Lander has had the most difficult assignment of all, and he has discharged it with skill and grace. The late fifteenth century has notoriously defeated historians because the evidence is both patchy and contradictory and because the framework fixed upon it by chroniclers and dramatists has proved exceptionally enduring. In the last thirty years, however, much work has been done, especially by Lander himself, to remove legends and establish truths. A new conspectus has been due for some time, and this is it. The supposedly exceptional violence, the near collapse of gov-

ernment, and the general air of despair, which used to dominate our view of the period, has been replaced by a reasonably "normal" story of political maneuvering among the aristocracy and gentry, increasingly cautious and hesitant as the crown reasserted its customary ascendancy. We now know also that the era experienced an economic recovery and increasing spiritual dissatisfaction, features that clearly prepared the ground for the dynamic expansion and religious reform of the sixteenth century. The Wars of the Roses, once seen as a bloody upheaval, have been reduced to the small-scale and intermittent fighting they were; government can be described as working well enough, inasmuch as the people supplied the needs of the crown, and courts administered as much justice as was customary in medieval England.

In the process the picture has lost its strong colors, some of which at least ought not to have been painted out. One needs to make an effort to feel the reality of those usurpations and murders in high places, which this sober account allows to happen almost by the way. Although Lander has no wish to hide the violence of court politics or the difficulties caused by an expanding population pressing upon inelastic resources, he tends to emphasize normality and to see his actors as reading the familiar lines of an orderly play. His severest strictures he reserves for Henry VII, made to appear more savage than strict and more bigoted than statesmanlike. This is possibly a necessary corrective to earlier admiration, but the case is overstated; dislike of the king's policies toward the nobility, described in the only tones of passion that this careful historian permits himself, distorts the story at its conclusion. Not that the author covers up the criminal follies of Richard III or the frivolous shortcomings of Edward IV; the age remains unexhilarating.

Before we read its story we are given, in the best part of the book, a remarkably complete and searching analysis of nation, government, economy, religion, and intellectual life; here new research brings its reward in many novel things as well as new insights derived from the skillful juxtaposition of matters usually treated in isolation. All in all, this is an excellent opening volume in what promises to be a cooperative history of real distinction.

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JOSEPH R. STRAYER. *The Reign of Philip the Fair*. Princeton: Princeton University Press. 1980. Pp. xvi, 450. Cloth \$35.00, paper \$13.50.

For a half century the reign of Philip the Fair has attracted the attention of some of the most able

medievalists in France and America. Their efforts have resulted in publishing the royal registers and accounts and in assembling a vast *fichier* of royal *actes* at the Archives Nationales, but only in more recent years have they begun to produce comprehensive and interpretative studies. Prominent among these is Joseph R. Strayer's long-awaited volume. By the term "reign" Strayer designates what French colleagues call *gouvernement*, and he is specifically concerned with the *rouages* or machinery of government, which he analyzes systematically from the central piston to the most minute cog. The parts that interest him most are the personnel, not exclusively the great nobles or the king's highest or most visible advisers but the nearly one thousand middle- or lower-level administrators who insured the government's smooth operation. After dividing them into four categories posted at Paris or in the provinces, Strayer has devoted his most original work to analyzing with masterful skill their recruitment, rewards, deployment, and functions.

Their effectiveness contributed to many advances in justice and finance, among which the articulation of the functions of the Parlement, the creation of national taxation, the employment of receivers, and the manipulation of currency evoke well-known characteristics of the emerging modern state. Moreover, these administrative achievements were matched by success in dealing with external forces. In a chapter that treats the highly publicized conflicts with Pope Boniface VIII and the Templars, Strayer minimizes Philip's disputes with the church and stresses an underlying policy of collaboration. Although the king's wars on two fronts in Aquitaine and Flanders brought him little profit and some notable military reverses, the royal government nonetheless expanded its borders and continued to extend its influence outside the royal domain. And most important, although the monarchy met some internal resistance, it succeeded for the most part in enlisting the loyalty of the politically vocal segments of the population through *enquêteurs-réformateurs*, petitions, and the evoking of assemblies. "If politics is the art of the possible," Strayer concludes, "then he [Philip] was a good politician."

To medieval contemporaries as well as to modern historians, the central enigma of Philip's reign has been to identify who was the driving engine behind this well-functioning machinery, the king himself, who appeared as handsome as an owl, but virtually mute, or his highly articulate counselors, Flote, Nogaret, Marigny, and others, who announced and implemented royal policies. Strayer's response to this question remains the same as in his famous article (*American Historical Review*, 62 [1956]: 18-32) and informs the entire structure of the present work: despite appearances, Philip was active in making the great decisions of his reign. Appropriately, the

title of every chapter in the book begins with the king. This answer is grounded on two kinds of evidence. A close scrutiny of the royal charters shows Philip personally involved with governmental routine and often with trivial affairs. If the king took interest in such minutia, Strayer reasons, he presumably dealt with important matters as well. In 1978, however, Robert-Henri Bautier (*Revue historique*, 525 [1978]: 3–27), working with the same evidence, came to opposite conclusions: the king scarcely governed but left the major tasks to his counselors. More persuasive, therefore, are Strayer's broader arguments based on circumstantial evidence. When he examines the activities of the counselors, he discovers that the characteristic royal policies preceded the appearance of his advisers and that their coordination and effectiveness can only be explained by a single mind directing from behind the scenes. Strayer's ultimate judgment approaches that of Robert Fawtier (1940) and most recently of Jean Favier (1978). Although the king used his advisers to declare and enforce different facets of his policy, in the last analysis he chose these men and governed through them.

In making a strategic decision to describe Philip's governmental machinery within topical chapters, the author has been obliged to sacrifice chronological development and climactic drama. We miss, for example, the full convergence of the crises of 1300–04 because the Flemish war, the confrontation with Boniface, and the governmental reforms are all relegated to different sections. Because military events are treated separately from administration, we are unaware that the royal council sometimes ruled alone because of the king's absence. Nor are we prepared for the calamities at the end of the reign culminating in the formation of the leagues—crises that brought down the Capetian dynasty a decade later. Having chosen to accentuate how well Philip's government worked, Strayer himself lacks words to account for its failures. In the end, however, Strayer's history of Philip the Fair is a lucid reflection of its subject. Like the king himself, it is highly successful, masterful, pragmatic, always relying on good sense, but still permitting a suspicion of mystery at its core.

JOHN W. BALDWIN
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FRANZ IRSIGLER. *Die wirtschaftliche Stellung der Stadt Köln im 14. und 15. Jahrhundert: Strukturanalyse einer spätmittelalterlichen Exportgewerbe- und Fernhandelsstadt*. (Vierteljahrschrift für Sozial- und Wirtschaftsgeschichte, Beihefte, number 65.) Wiesbaden: Franz Steiner Verlag. 1979. Pp. viii, 413. DM 88.

Over the past decade or so there has been a new enthusiasm for the study of medieval Cologne. Old concerns have received fresh analysis; new concerns, which sometimes seemed too formidable to attempt, have been attacked with relish. Franz Irsigler has made a major contribution to this enterprise. His work, like that of most of the others, emphasizes economic and social life in the later Middle Ages. The book under discussion here is a synthesis of much of his earlier work and is a splendid contribution.

The author has painstakingly exploited archival and printed sources. He seeks to be as concrete as possible, to quantify his material as far as the sources will allow; but he also shows a keen awareness of the limitations of the sources. The extant series of *Stadtrechnungen*, especially important for statistical material, begins only in 1370; other sources, notably the excise books, come primarily from the period after 1450. Naturally, then, Irsigler emphasizes the fifteenth century, even the latter half of the century.

Care in the use of sources does not prevent the author from reaching significant conclusions. Analyzing in detail the structure of the three major branches of the Cologne economy (textiles, metal products, and dressed leather and furs) as well as two other areas (beer and wine and spices), he concludes that Cologne retained its preeminent position among German cities in craft production and long-distance trade into the sixteenth century.

He attributes Cologne's position to the diversification of its production for export, where a decline in one area was often compensated for by growth in another, and to the close connection of such production and long-distance trade. A decline in some areas of basic production was offset by a growth in finishing industries. Even the decline of the woolen industry in the fifteenth century was offset by the rise of new textiles: fustian and silk.

Centralized control of enterprise also contributed to Cologne's success. In industries such as woolens and iron, master craftsmen gained control of raw materials, production, and marketing; in others, such as fustians and silks and nonferrous metals, merchants did the same. The author believes that such concentration represented necessary progress if Cologne was to remain competitive in the later medieval economy. Convinced of this conclusion, he becomes rather shrill at times in insisting that the disadvantages of this concentration (artificially depressed wages, a truck system of payment, and so forth) have been emphasized too much in the literature.

The value of the study is enhanced by the author's emphasis on the economic relationship of Cologne and the surrounding countryside. Through the work of scholars like Irsigler, medieval Cologne

is finally receiving the careful attention it deserves.

PAUL W. STRAIT
Florida State University

HENRYK ŁOWMIAŃSKI. *Religia Słowian i jej upadek (w. VI–XII)* [The Religion of the Slavs and its Decline from the Sixth to the Twelfth Centuries]. Warsaw: Państwowe Wydawnictwo Naukowe. 1979. Pp. 432. 100 Zł.

This book is a study of the character of Slavonic religion and an analysis of why Christianity eventually came to supplant native pagan practices. Although closely related to the author's long years of research on the origins of Poland, this book provides a picture painted on a broader canvas than his multivolume *Początki Polski*. Henryk Łowmiański's purpose is to present a comparative study of all Slavonic religion to show both the unity and the diversity of this phenomenon and to point out thereby the importance of the adoption of Christianity. (He does not concern himself much with those instances where the new religion was forcibly imposed upon conquered groups.) This is an ambitious task, which in this century has attracted such great scholars as Niederle, Brückner, and Jagić. It is also a difficult task, for the data that bear upon the nature of native religion are fragmentary and often ambiguous. Despite these problems of scope and method, this present work is on the whole successful.

The opening prolegomena of seventy pages is devoted to methodology and historiography. In the second section of more than 150 pages, Łowmiański describes in turn the religions of the Southern, Eastern, Polabian, and Western Slavs. He is exhaustive in his utilization of the literary and archeological sources for this subject, but he makes little use of some of the recent models provided by comparative mythology. He rejects, for example, the trifunctional reconstruction of the Slavic pantheon in the early Middle Ages that some scholars, most recently Aleksander Gieysztor, have derived from the work of Georges Dumézil. Thus, the description Łowmiański provides here will strike many as quite traditional. The final, slightly longer section explores the legal, territorial, and eschatological elements that successfully enabled Christian missions among the Slavs to achieve the conversion of the various groups. In the confrontation between two religious systems—a traditional group religion and a universal religion with sophisticated doctrine and an ethical system that transcended the patterns of the local group—it was the latter that both the people and the ruling groups found most appealing and useful. This description is also relatively traditional, but Łowmiański's broad learning and schol-

arship make the richness of presentation more subtle and insightful than previous work has been. A useful index of gods and mythological figures (including variant forms of names) complements the book.

PAUL W. KNOLL
University of Southern California

MODERN EUROPE

WINTHROP S. HUDSON. *The Cambridge Connection and the Elizabethan Settlement of 1559*. Durham, N.C.: Duke University Press. 1980. Pp. x, 158. \$14.75.

The young Queen Elizabeth I informed the Spanish ambassador of her resolve "to restore religion as her father left it." The effect of this book is to restore with embellishments that understanding of the Elizabethan religious settlement that prevailed before 1950, when the late Sir John Neale unveiled his brilliant but hypothetical reconstruction of the parliamentary maneuvers of 1559. According to Neale, the Elizabethan Act of Uniformity was not within the original legislative intentions of the queen and her advisers but emerged from a partisan engagement in which those Protestant forces that were to confront the queen in later parliaments were already effectively deployed to frustrate her conservative intentions. Winthrop S. Hudson believes that not only was the act in line with policies devised before Parliament met but also that during the parliamentary debate the government never lost control of strategy and timing.

In rejecting what was perhaps an overly ingenious reading of the cryptic public record, Hudson makes no attempt to match the meticulous, microscopic parliamentary scholarship of Neale himself. This distinguishes his study from the critique of Neale made independently and almost simultaneously by Norman L. Jones in his 1977 Cambridge Ph.D. thesis, "Faith by Statute: the Politics of Religion in the Parliament of 1559." Jones does not support Hudson's impression of effective governmental control of parliamentary time. He found that no less than twenty days were lost before Easter, 1559, in the investigation of disputed titles to episcopal lands. Hudson does not mention those twenty days. In an approach that is more extensive than intensive he sets the Elizabethan settlement against its intellectual and human background. The "Cambridge Connection" of the title was that talented generation of mid-Tudor graduates who brought to fruition in civil and ecclesiastical administration the values learned in the newly reformed university. Hudson suggests that the Elizabethan settlement was consistent with those values and precisely the kind of settlement that we should expect Cecil, Ba-

con, and Parker, "the Elizabethan Establishment," to have devised.

As a political explanation of how the Elizabethan settlement was made, the potential of this insight is somewhat blunted by Hudson's insistence that Queen Elizabeth herself shared the outlook of "the Connection" and did not need to be persuaded or maneuvered. Since there is some evidence of unresolved religious tension to the contrary, this attempt to restore the centrality in early Elizabethan politics of a monolithic government is retrogressive. Neale erred in finding a little more in the story of 1559 than the record could easily bear. Hudson seems to err in the opposite direction, by glossing over awkward but significant fragments of evidence pointing to the story within the story.

PATRICK COLLINSON
University of Kent

PATRICK COLLINSON. *Archbishop Grindal, 1519-1583: The Struggle for a Reformed Church*. Berkeley and Los Angeles: University of California Press. 1980. Pp. 368. \$27.50.

This is the best book yet written by the leading historian of Elizabethan puritans. Although Patrick Collinson has done much more than respond to such a need, the fact is that a study of Edmund Grindal is long overdue. It has been an absurd necessity to turn to the compendium produced by the industrious eighteenth-century vicar, John Strype, for information about the man probably second in importance only to John Foxe in the first generation of the English Protestant church. Successively bishop of London, archbishop of York and Canterbury, Grindal was also a major intellectual balance wheel in the feverish whirl of dogmatic gears that characterized Elizabethan Christianity. He remained a towering figure in the subsequent history and lore of the English church in the revolutionary episodes of the next century. The later trivializing and anachronistic writing about Grindal—and there has been a long shelf of it—can now be completely discarded for Collinson's magisterial account.

Edmund Grindal was born in the remote Cumbrian village of St. Bees among some of the poorest farmers in all England, of whom his father was one. The family inheritance in 1570 was worth less than one pound a year in rent. It is interesting to consider the effects on the archbishop of his native "lawless country," where poverty and ignorance reigned over a people whom he declared "most oppressed of covetous landlords." Collinson believes Grindal's mind to have been cast in this Cumbrian mold: "he regarded poverty, the oppression of landlords, ignorance, irreligion, disobedience and law-

lessness as related symptoms of a general disease in the body politic, for which the only remedy was preaching and education." That is to say, Grindal shared the "commonwealth's men" mentality that Collinson (rather carelessly) regards as "virtually a national conviction" by the end of the Tudor century. But for all such socially conscious sentiments, Collinson properly emphasizes that Grindal was "incapable of a radical or even rational analysis of existing social arrangements" and that he never recognized any root relationship of economics to social or cultural realities.

Collinson's greatest narrative accomplishment is the re-creation of the complex setting of international heretical Christianity that shaped Grindal's career and the church in England that he led. In a fine chapter on the Edwardians, he convincingly distinguishes Grindal from "the many lesser Bezas" of the next generation of witch-hunters and follows that with a perfectly marvelous evocation and account of that most complex and compromised phenomenon, the Elizabethan settlement. Grindal as bishop of London—recruiting a ministry of some 294 in his first two years while confronting the hectic and frightening politics of probably the most important diocese in all Christianity—is examined with patient authority and imagination.

Collinson, premier historian of radical religious dissent, presents a masterly and admiring portrait of the perplexing world of a bishop of adiaphorism and latitudinarianism. Grindal ordained John Field, "the Lenin of Elizabethan puritanism"; but it was he who warned Cecil of the dangers of Thomas Cartwright's "busy head stuffed full of singularities." Never was he the feeble temporizer of historiographical tradition. He was a true exponent of the "Germanical natures," courageous and disciplined. The celebrated crisis over his refusal to obey the queen's command to suppress prophesyings was no monument to futility; his most passionate belief was that Christianity could best express itself through that quality of preaching that the prophesyings were intended to produce. His fight was absolutely central to Protestantism and a vindication of his own integrity. Cecil had appointed him for such presumed character and dedication; most of his bishops agreed with him. To say, as Collinson does, that Grindal was "a perfect fit with consensus, sharing its protestant mind" is to flatter Protestantism. If Grindalians were not to prevail over Laudians in future generations, the fault was not that of the "Apostle of the North."

C. H. GEORGE
Northern Illinois University

DAVID CRESSY. *Literacy and the Social Order: Reading and Writing in Tudor and Stuart England*. New York: Cambridge University Press. 1980. Pp. x, 246. \$29.50.

During the 1960s Lawrence Stone, in a series of articles on education, literacy, and social mobility (*Past and Present*, 1964, 1966, 1969), asked some new questions about English society during the Tudor and Stuart period. In his thesis on "The Educational Revolution, 1540-1640," Stone argued that high literacy rates in England helped to explain many aspects of the period, not least the extraordinary flood of pamphlets in those years, the passionate advocacy of Bible reading, and the rise of a sophisticated movement of political radicalism. David Cressy's book, *Literacy and the Social Order*, published a decade and many printouts later, probes some of Stone's generalizations and provides, thanks to sophisticated computer analysis of diocesan and other records, elaborate statistics about rates of literacy among the various social groups of Tudor and Stuart England.

The important question is, what remains of the "Educational Revolution?" The answer seems to be, a great deal, although Cressy suggests certain modifications of the original thesis. His statistical analysis points to an early growth of literacy in the 1520s and 1530s, a surprising decline during the years of Edward VI's reign, a boost during the first half of Elizabeth's reign, a decline during the crisis years of the 1590s, and further growth under the early Stuarts. The years of civil war brought further deterioration lasting substantially until the 1690s, when, particularly for the inhabitants of the London area and including for the first time large numbers of women, the trend toward literacy once again accelerated.

In seeking explanations for all this Stone speculated boldly. Cressy, in contrast, urges caution. He criticizes Stone's suggestion that there was a marked difference between the literacy rates of the highland zone and the lowland zone. He also fails to discover any correlation between high literacy rates and the presence or absence of Puritan ministers or the proximity of schools, which Stone had stressed as possible causes. We have clearly reached a stage in the historical study of literacy familiar in other areas of social history when the conquistador is succeeded by the computer. Where do we go now? The analysis of local market areas, on lines suggested for China by William Skinner, may provide a fruitful approach to explaining why some areas advance toward literacy more rapidly than others. We need perhaps to make a distinction between types of literacy. There may be a key difference between "salvation literacy" and "market literacy." Such approaches might well mean supplementing the statistical with other sources of evidence and breaking away from the units in which the material was assembled originally. The work of anthropologists, such as Jack Goody, deserves attention. Cressy's work is very impressive, but it would be a pity if it

marked a gravestone and not a milestone. The "Education Revolution" deserves a better fate than the "Gentry Controversy."

HUGH F. KEARNEY
University of Pittsburgh

CHRISTOPHER HILL. *Some Intellectual Consequences of the English Revolution*. (Merle Curti Lectures, 1976.) Madison: University of Wisconsin Press. 1980. Pp. 101. Cloth \$13.50, paper \$4.95.

J. G. A. Pocock, editor. *Three British Revolutions: 1641, 1688, 1776*. (Folger Institute Essays.) Princeton: Princeton University Press. 1980. Pp. ix, 468. Cloth \$32.50, paper \$12.50.

Three British Revolutions consists chiefly of papers presented at a Folger Institute symposium in May 1976. Nearly all are of high standard. Lawrence Stone provides a provocative survey of the "seismic rift" in seventeenth-century English politics, Christopher Hill a cogent restatement of his "bourgeois revolution" thesis, Gerald Aylmer a useful re-examination of the transformation of local and national elites in 1640-60, and Robert Ashton one more reminder of the conservatism of mainstream parliamentarians. Essays by Lois Schwoerer and David Lovejoy illuminate the English and colonial dimensions of 1688. The eighteenth-century essays include J. G. A. Pocock's thoughtful analysis of political and ideological interactions, Alison Olson's closely reasoned examination of the reasons why English legislation became harder to enforce in the colonies after 1750, John Brewer's impressive discussion of the British radicals (focusing on their paradoxical employment of "Country" rhetoric), and John Murrin's interesting attempt to reconcile ideological and social interpretations of the revolutionary decades in America. Only one essay seriously disappoints: Charles Carlton's on Charles I, a piece of shallow psychohistory interwoven with juvenile historiographical and political commentary.

The better papers provide excellent introductions to the more extensive works their authors have published elsewhere. They might well whet the appetites of students; they certainly offer a stimulating source of mental dialogue for the informed reader. Whether they demonstrate, as the editor claims, the emergence of a new perspective, even a new paradigm, linking all three revolutions, is less clear. In one sense they do, for throughout we are reminded of the persistence of the "Court-Country" divide in all its mutations, from the beginning of Stone's "seismic rift" to Murrin's "Great Inversion" in Jeffersonian America. There is something approaching a consensus (not shared by Hill or, to a lesser extent, by Brewer or Murrin) that all three revolutions

must be interpreted as the result of governmental breakdown at the top, rather than as the product of conflict between social groups or of change from below. To be sure, nearly all the writers make the customary bow to the commercial revolution, but the preoccupation with the political culture of the elite is unmistakable. This is in striking contrast to some of the papers presented at a more recent symposium (sponsored by the Institute for Research in History in November 1980), in which the Americanists at least showed much greater interest in popular movements and their social origins.

As already noted, the clearest exception to Pocock's consensus is provided by Christopher Hill. In his brief *Some Intellectual Consequences of the English Revolution*, based on his 1976 Curti Lectures at Madison, Wisconsin, Hill utters another sharp dissent. The lectures amount to a breathless and oversimplified recapitulation of points that Hill has made elsewhere and often stray from the intellectual themes suggested by the title. It would be easy to argue with specific points: with the "bourgeois revolution" concept, for example. But it is more useful to acknowledge the service Hill performs by stressing the unintended consequences of the 1640s, which were in the long run as central to the Anglo-American tradition as the intended ones. Among the latter are of course numbered the notions of constitutionalism and ordered liberty so often stressed by Pocock's other contributors; but it is hard to see the forces that produced an individualistic society based on market relations and an emerging class system as any less worthy of attention. We may differ on the precise role played by the English Revolution in this social transformation. But no new political paradigm, however persuasive, need exclude discussion of the process, or deny the significance of Hill's other central theme, the defeated alternatives proposed by the radical revolutionaries. Both revolutions and stable governmental systems are the product of social as well as political forces.

DAVID UNDERDOWN
Brown University

STEPHEN SAUNDERS WEBB. *The Governors-General: The English Army and the Definition of the Empire, 1569-1681*. Chapel Hill: University of North Carolina Press, for the Institute of Early American History and Culture, Williamsburg, Va. 1979. Pp. xxi, 549, \$22.50.

It is Stephen Saunders Webb's contention that an obsession with commercial motivation has resulted in consistent neglect of the military's role in the establishment of the first British empire. In this book, the harbinger of a projected multi-volume study, he

seeks to revise the accepted account of the beginnings of colonial expansion. The three sections of the work focus upon the development of "garrison government" in the British Isles (1569-1676), Jamaica (1654-1681), and Virginia (1676-1683). This is a key concept for Webb, and by it he understands not merely the physical presence of the soldiery, but the expansionist, centralizing, and authoritarian political priorities to which he believes the English officer and bureaucratic cadres increasingly subscribed. "Garrison government," he concludes, "formed an empire." The point is driven home by a prosopographical appendix emphasizing the military background of the provincial governors appointed between 1660 and 1723.

The book is eminently readable, despite its complex structure, and, in its impressive combination of ambitious sweep and detailed erudition, most richly textured. Old problems are clarified by Webb's revisionist perspective. The fears of executive aggrandizement that so exercised colonial theorists emerge not as academic variations on themes struck by Polybius and Harrington but as responses to a stark, tangible threat. Yet the work is flawed; its merits are hostage to the untenably rigid central hypothesis that Webb, in his eagerness to chastise traditional historiography, seeks to maintain.

The firm distinction that Webb draws between commerce and military power in the development of the empire is surely a false dichotomy. The circumstances behind the passage of the third Navigation Act (1673), for example, and the appointment of Edward Randolph (1678) demonstrate that mercantile concerns were not the sole preserve of opponents of the Caroline court. Neither of these events is discussed in the book and, in general, evidence that might suggest the commercial thrust behind imperial expansion is simply passed over. Moreover, Webb's presuppositions blind him to the full significance of material that he has discovered. It appears from his thorough analysis of Jamaican developments that some governors were always sensitive to commercial needs, while others, transmuted by provincial circumstances and pressure groups, became increasingly involved in, and concerned about, trade.

Webb's suggestion that a transcendent military-imperial ethos infused the court of Charles II similarly depends upon a neglect, both passive and active, of countervailing evidence. Confident assertions that "provincial proconsuls" had "steadily increased their power relative to that of the traditionally dominant local elites" (p. 55) rely upon a questionable juxtaposition of analyses of a couple of Tudor examples and of the extraordinary developments of the civil war and interregnum; they disregard the recent scholarship that has dissected the "normal" mode of English government through

1660. Webb's own research demonstrates that a trail of jobbery and corruption, intrigue and inefficiency marked the passage of Charles II's government, yet this is glossed over in his efforts to establish the existence of a coherent imperial ideology. So in 1666, Webb argues, the king "adopted the paramilitary instruments of sovereignty developed in the Interregnum and epitomized in the office of governor" (p. 106). The particular incident that provoked this rhetorical flourish was the appointment of Lord Culpeper, impecunious and unreliable, as governor of the Isle of Wight. Before selling his office, Culpeper complacently presided over the collapse of his fortifications and the desertion of his garrison, pocketing the revenues earmarked for the maintenance of both. Verres, we should recall, was a proconsul—not just Pliny.

Yet, if Webb's master thesis is marred by hyperbole and a partial reading of the evidence, his achievement is still very considerable. His research has enhanced our detailed knowledge of colonial expansion; his challenges have ensured that no future historian of the first empire can invoke commercial interests as the sole motivation for its growth.

CLIVE HOLMES
Cornell University

PETER ROEBUCK. *Yorkshire Baronets, 1640–1760: Families, Estates, and Fortunes*. (University of Hull Publications.) New York: Oxford University Press, for the University of Hull. 1980. Pp. xiv, 414. \$65.00.

The history of the English landed aristocracy in the period leading up to the Industrial Revolution has been a topic for a good deal of intelligent speculation. In shorter supply, however, are general social or economic facts to support such speculation, and it will require a great deal of hard work if this is ever to be changed. Peter Roebuck confines himself not merely to one restricted group in one county, the 93 baronet families of Yorkshire, but devotes half his book to detailed studies of only four of them, the Hothams, the Beaumonts, the Constables, and the Brights. He also makes numerous observations about the other 89 families, without much attempt at systematic detail.

His main conclusion, not surprisingly, is that diversity of experience was great, even among what might well seem a homogeneous group. In particular, the succession of a new baronet often meant a complete change in the policies and fortunes of the family, due to his personal character.

The system of strict marriage settlements was tremendously important, as has often been suggested. A bride brought a dowry to her husband's family, against which a specified widow's income was

promised for her. If she died first, the husband's family thereby made a clear profit, and indeed several successive such marriages might well occur. A long widowhood would cripple a family, however. When the father had remarried late in life, and his son died young soon after his father, moreover, an estate could be saddled with two such jointures to pay simultaneously. With high mortality risks and no insurance, the results were bound to vary enormously. Sir Richard Beaumont's marriage to an heiress seemed, at the time, the guarantee of the Beaumont fortunes; but the terms were necessarily favorable to the lady, and he died shortly afterwards. His widow, despite her remarriage to an earl, drew her pension for 26 years. Sir John Bright, to give another example from one of Roebuck's four selected families, drew £3,210 a year from his rents; after his death, his heirs received £900 a year less because they did not inherit all the property, but were still obliged to pay Sir John's widow £1,000 a year for 49 years and Sir John's daughter-in-law £400 a year for 20 years.

The book shows clearly how difficult it is to draw valid general conclusions even when records are plentiful. It is an admirable guide both to the kind of information that can be uncovered by research and the difficulties of doing so. The references and notes are copious, and it makes an excellent piece of small-scale social history.

T. H. HOLLINGSWORTH
Glasgow University

DENNIS R. MILLS. *Lord and Peasant in Nineteenth Century Britain*. (Croom Helm Historical Geography Series.) London: Croom Helm or Rowman and Littlefield, Totowa, N.J. 1980. Pp. 232. \$29.50.

This is a strange book. The very title, with its implication of central European feudalism, is intriguingly odd, and Dennis R. Mills—while acknowledging its inherent difficulties—never satisfactorily explains it. When, toward the end of the book, the "urban peasant" appears, semantic choices become incomprehensible to a historian. One's mind boggles at the thought of "peasants" toiling in the mills of Manchester, Bradford, or Glasgow. Where, one wonders, are sociologists and historical geographers trying to lead us? It seems to me that (*pace* Disraeli, who once wondered "what can it signify . . . whether a man be called a labourer or a peasant?") the title is unfortunate.

What Mills is actually concerned with is not some medieval hangover but some (highly selective) aspects of Victorian relationships. This is a fascinating topic, about which we need to learn a great deal; but we do not learn it here. Mills tends to overstress dichotomies and dualities—arable and

pasture, champion and hamlet England, land and industry, *Gemeinschaft* and *Gesellschaft*, "open" and "closed" villages, estate and peasant systems—and attempts to demolish some theories about them. His method is to use case studies of particular village experiences. These are interesting enough but would be more appropriately sited in local journals. Actually, most studies are based upon papers in regional publications; there is little original research here.

Mills certainly challenges nonhistorical assumptions about the *mores* of "paternalistic" aristocrats. But surely, after the work of the pioneers David Spring, F. M. L. Thompson, Graham Mee *et al.*, he is tilting against decomposed windmills. Do we not now accept the positive role of landowners in industrial and other nonagricultural enterprises—turnpikes, canals, railways, coal, iron, lead, lime, tin, ports, harbors, urban development, even banking and textiles? Mills simply uses a few well-known examples.

Some well-researched case studies of village experiences are quite fascinating, and antiquarians may find them useful aids to parochial histories of Thurlby-juxta-Norton and its like. But historians will not be surprised to read that estate cottages were superior, that tenants tended to vote for the estate "interest," that agricultural unions opposed farmers, and that British land was largely owned by a few hundred families. They will, however, note Mill's simplistic belief in the "politics of deference," misunderstanding of trade unionism, and errors on Scotland.

Mills forecasts an end of "large-scale monoculture" and a revived "peasantry" in a deindustrialized society, quoting the leader of the Ecology party, whose candidates were treated with universal derision at the general election of 1979.

This is not a history book.

J. T. WARD
University of Strathclyde

BARRY GORDON. *Economic Doctrine and Tory Liberalism, 1824–1830*. London: Macmillan Press or Barnes and Noble, Totowa, N.J. 1979. Pp. xii, 181. \$25.00.

Barry Gordon, an Australian economist, has produced a fine study in *Economic Doctrine and Tory Liberalism, 1824–1830*. During the years between 1819 and 1830, certain leaders of the Tory party fell under the influence of Ricardian political economy. Gordon has already discussed the first five years of this period in an earlier volume; here he turns to the confrontation between disciples of the new system of nonintervention and free trade and a group of merchants and bankers who were not prepared to move quite so fast or so far.

Gordon ably describes this conflict as one between sectarian doctrine and practical experience. The merchants, even though they understood the advantages of a freer trade, resisted the efforts of the Ricardians, who were so confident of their "science" that they were prepared to follow wherever its deductions might lead. Somewhere between these two groups, the Tory liberals—notably Liverpool, Huskisson, and Peel—were ready to demand tariff reciprocity and even some measures of protection.

The Panic of 1825, attributed by contemporaries both to monetary causes stemming from the 1819 resumption of specie payment, as Ricardo had advised, and to the freer trade policies of the Tory government, was a blow to the political economists. The 1824 repeal of the Combination Acts, followed not by the disappearance of trade unionism and labor violence, as its laissez-faire sponsors had predicted, but by an increase in both, was also a setback for the Tory liberals. Interestingly enough, however, even the political economists were ready to admit the need for governmental intervention in Ireland to overcome structural obstacles to economic growth.

What Gordon describes as a "protectionist backlash" pitted the practical-minded businessmen—led by the banker Alexander Baring, himself not unsympathetic to the new science—against the president of the board of trade, William Huskisson, who although not a sectarian was an earnest fellow traveler. In the twenties, the spokesmen for the commercial community opposed Huskisson's proposals to promote a freer export of machinery and objected to the lifting of protection for the silk and shipping interests. Surprisingly, the duke of Wellington, whom historians have set down as an enemy of the liberal spirit, supported freer trade policies as prime minister.

Gordon might have pursued more fully the economic ideas of the opponents of the Ricardians during this period. Although he does well by the Attwoods of Birmingham and their opposition to the deflationary gold standard, as well as by the labor-value socialists, he slights the Adam Smith–Malthus school. The arguments of this school, which was wholeheartedly supported by the Tory *Quarterly Review*, underlay the agrarian defense of the Corn Laws.

Gordon has sharpened the focus of F. W. Fetter's recent, century-long survey of the parliamentary activities of the political economists, and has done his job well. Historians must welcome his effort and look forward to a continuation of his study into the 1830s and 1840s.

BERNARD SEMMEL
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Stony Brook*

EDWARD ROYLE. *Radicals, Secularists and Republicans: Popular Freethought in Britain, 1866-1915*. Manchester: Manchester University Press or Rowman and Littlefield, Totowa, N.J. 1980. Pp. xii, 380. \$29.50.

This book is the natural continuation of Edward Royle's earlier work, *Victorian Infidels* (1974), and completes his authoritative history of the secularist movement in Britain. As the former volume centered on the career of George Jacob Holyoake, so the second is dominated (literally, for he was physically a huge man) by Charles Bradlaugh, described most commonly as "formidable" and "indomitable" by his contemporaries. He was indeed a hammer, and, as Royle remarks, his pseudonym "Iconoclast" was well chosen.

In the first part of his study, Royle traces the fortunes and outlines the chronology of organized freethought between 1866 and 1915. The National Secular Society was founded in 1866, amid the radical agitation that led to the Second Reform Act of 1867. After a slow start the movement was reorganized in 1874 and enjoyed a brief golden age from the Knowlton trial (of Bradlaugh and Annie Besant for publishing the birth control pamphlet, *The Fruits of Philosophy*) in 1877 to Bradlaugh's admission to Parliament in 1886. Thereafter the N.S.S. declined in membership and size of activities. At its peak the membership was less than 4,000, but the number reached through lectures and publications was of course much larger.

Part 2 describes the anatomy of the freethought movement at the national and local levels. Royle provides some very useful thumbnail sketches of the top leadership: Bradlaugh, Holyoake, Besant, Charles Watts, George William Foote, and William Stewart Ross. Even more fascinating are the biographies of lesser leaders like Harriet Law, Joseph Mazzini Wheeler, and Chapman Cohen. Something of the tone and temper of secularism in the branches comes through in the details of the thriving Leicester Secular Society, with its impressive hall in the Flemish Renaissance style, its Sunday services, amateur dramatics, cricket club, and general chapel-like activities.

For many readers part 3 will be the most rewarding, for here Royle charts the intricate relationships between freethought and other reform movements, fully justifying the title of the book. There is much valuable information on land reform, socialism, female emancipation, blasphemy, and education—all from the rather unusual angle of freethought. What Royle does, and does extremely well, is set secularism in the context of a variety of later nineteenth-century social and political reform movements, thereby demonstrating that such movements and beliefs are most fruitfully studied not by themselves but in clusters. Recent work by historians and soci-

ologists of religion has emphasized the similarity between secularism and Protestant sectarianism. Royle's study makes clear that secularism was no less concerned with politics than with religion and must be interpreted as an integral part of the history of popular radicalism, stretching from Thomas Paine to the modern labor movement.

JOHN F. C. HARRISON
University of Sussex

RICHARD PRICE. *Masters, Unions and Men: Work Control in Building and the Rise of Labour, 1830-1914*. New York: Cambridge University Press. 1980. Pp. xi, 355. \$34.50.

In this study of industrial relations in the English building trades, Richard Price sets out to remedy "the inadequate level of conceptualisation and theoretical perspectives that has been the most serious omission of mainstream labour historiography." He cites Henry Pelling as an example of what he considers the inadequacies of the received interpretation: "Inconsistent even in the explanations that it does adduce to explain the rise of labour, traditional labour history is a lode of interesting and important facts crushed together to hagiographically confirm Labour's own mythologies" (p. 1). As a means of escaping from "this rather depressing landscape," Price commends the methods of the new social history, especially as manifested in the revisionism of John Foster, Robert Gray, and James Hinton. The theme of his book is the fundamental importance of the work experience as a "structural determinant of labour history."

Despite extensive research and a strenuous effort to provide a more analytical treatment of "the rise of labour," Price has been less than successful in his ambitious undertaking. A major difficulty lies in the abstractness and imprecision of his concepts—a collage of industrial sociology and populist Marxism. This weakness is compounded by the broad chronological and geographical scope of the book. As a result, Price's study lacks the concreteness characteristic of the monographs by Foster and Gray, who applied a coherent Marxist sociology to the analysis of working-class behavior in its social, institutional, and historical context. Price has not been able to "restore rank and file actions to their rightfully central role in labour history," because such actions are removed from their context and introduced to illustrate general patterns. We learn very little about work experience. When workingmen do make an appearance, their "rhetoric" is counterposed to an abstract "reality" defined in terms derived from industrial sociology. While such concepts as conflict and control, power and authority, and the structure of militancy are poten-

tially useful to the historian, they have to be linked to precisely specified particulars, especially when referring to the vast changes that took place in Victorian and Edwardian England. But Price is pre-occupied with only one kind of change. He believes that "revolutionary consciousness which moves beyond a resistance to 'encroachments' to challenge the social basis of society" grows out of the struggle for control in the workplace. This warmed-over version of the myth of the revolutionary proletariat does not provide a useful basis for a reinterpretation of English labor history.

In his zeal to provide a total revision of older interpretations, Price has done something less than justice to the work of his predecessors without presenting an alternative that reflects the analytical strength of the new social history.

TRYGVE R. THOLFSEN
Teachers College,
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JOHN BURNETT. *A Social History of Housing, 1815-1970*. Illustrated by CHRISTOPHER POWELL. London: David and Charles. 1978. Pp. viii, 344. \$35.00.

At long last we have a history of English housing that carries the story right up to the present period. In his well-researched and well-written account of *A Social History of Housing: 1815-1970*, John Burnett sets out to describe the types of dwellings occupied by the working and middle classes, to measure and evaluate changes in the quality of housing, and finally "to seek an explanation of the determinants of built form" (p. vii). He accomplishes his objectives very well.

Although Burnett does not break new ground in his treatment of nineteenth-century urban housing, he presents a clear and concise synthesis based upon the standard primary and secondary sources. He is especially effective in his analysis of the middle-class house. According to Burnett, the middle-class choice of a house was less dictated by rational concerns like size, location, and cost than by Victorian values of privacy, respectability, social identification, comfort, and accumulation.

Burnett balances his treatment of urban housing with an excellent analysis of cottage homes and the general housing of the rural workers. Despite the publicity given to the deplorable condition of the laborers' "rustic" cottages, Burnett argues that economic and financial considerations militated against significant improvement in rural housing until the end of the First World War.

Burnett does break new ground with his chapters on housing in twentieth-century England. He emphasizes the importance of the 1918 report of the Tudor Walters Committee, which gave its stamp of

approval to subsidized local authority housing in low-density suburban estates. But the housing problem was not solved in the interwar years. Burnett suggests that private builders and local authorities failed to provide sufficient dwellings at a level of rent affordable to the poorer workers.

According to Burnett, the post-World War II period witnessed a dramatic increase in house building due to full employment and a rising standard of living. He sees the most significant improvement occurring in the houses of the working classes; the semi-detached house in a low-density development became much more available to the working classes. Because of the reduction in the size of the family and the disappearance of domestic servants, this post-World War II period saw a reduction in the size and number of rooms in the average middle-class house. Thus working-class and middle-class house types have converged since World War II. Burnett concludes that for the majority of the English people, the ideal still remains the individual house in a garden.

Burnett's perceptive study of the social history of English housing is a welcome addition to the literature; it will be of interest to specialists in the field as well as to graduates and undergraduates.

NEIL L. KUNZE
Northern Arizona University

JOHN D. FAIR. *British Interparty Conferences: A Study of the Procedure of Conciliation in British Politics, 1867-1921*. New York: Clarendon Press of Oxford University Press. 1980. Pp. x, 354. \$49.00.

The problem with this generally excellent book is that it adds new layers of detail to questions that have always seemed difficult enough. John D. Fair's premise, although he never quite states it, is that interparty conferences may represent a new legislative forum. But the book is, in reality, a collection of separate essays in great depth describing the origin, progress, and results of a dozen meetings or conversations among politicians in the half century between 1867 and 1921, some of which were interparty conferences and some of which were not. The reader is immediately faced with a conceptual dilemma. On the surface Fair's intent is simple enough. An interparty conference, he says, represents an effort by opposing political parties to solve, by private negotiation, a legislative or constitutional question that is not amenable to resolution through the ordinary public processes at Westminster Palace. It is a parliamentary court of equity, designed to circumvent the rigidities of ordinary law. But the reader finds, as presumably Fair discovered when he looked into it, that the term "interparty conference" virtually eludes definition, that there was no

orderly, progressive evolution of the conference idea, and that, indeed, some conferences were not meetings at all. As a result, this book is not a history of the steady growth of the conference principle, but a series of extremely useful, although almost unconnected, essays describing certain political crises that were resolved, or not resolved, by negotiation.

Because of the period covered, many of the essays, from the disestablishment of the Irish church in 1867 to the treaty in 1921, deal with Ireland. In addition to these topics, Fair discusses the Constitutional Conference of 1910, the Buckingham Palace Conference of 1914, Lloyd George's attempt to win Home Rule in 1916 after the Easter Rebellion, the Convention of 1917-18, and the Meetings on Federal Devolution in 1919-20 within the Irish context. There are studies also of the redistribution agreement of 1884, of the attempt to compromise with the House of Lords over the Education Bill in 1906, of Lloyd George's ascent to the premiership in 1916, and of the conferences on electoral and House of Lords reform between 1916 and 1918. Some of these occurrences are hardly interparty conferences if those words connote formal assemblies of representatives of the Liberals, Unionists, or Labourites sitting down together. Sometimes the opposing factions never met. Sometimes the opposing groups were hardly political parties.

Nevertheless, as a series of essays re-examining a dozen familiar, critical encounters since the first Gladstone government, Fair has done an excellent job. He has seen all the documents and read the secondary accounts. His story of the fall of Asquith is the clearest and most sensible of the numberless accounts of that complicated episode. Even if it is essentially a source and reference text, this is a valuable book.

BENTLEY B. GILBERT
University of Illinois,
Chicago Circle

STUART MACINTYRE. *Little Moscows: Communism and Working-Class Militancy in Inter-War Britain*. London: Croom Helm. 1980. Pp. 213. \$30.00.

The interwar period is at last beginning to attract from British labor historians the sort of attention that has been lavished for so long—and with ever-diminishing returns—on the late nineteenth and early twentieth centuries. Here by contrast is an almost untouched field, rich in resources, yet in many respects something of a historiographical wasteland. Stuart Macintyre sets out to uncover the dynamics of working-class life in some of those communities whose militancy during the interwar years was such that they earned for themselves the epithet of "Little Moscows." Taking as his subjects Scotland's

textile-dominated Vale of Leven and the mining villages of Mardy in South Wales and Lumphinnans in Scotland, he shows that their spirited response to the pressures imposed by industrial contraction and mass unemployment grew out of communities largely composed of manual workers and with a high degree of integration in terms of family ties, friendships, and recreation. While he is rightly too cautious to expand this into a general explanation applicable to other contemporary militant centers, there is here a very apparent contrast with the more diversified working class, say of Lancashire, where reaction to the depression was relatively muted. Mardy, Lumphinnans, and the Vale of Leven were distinctive in that their unemployed never surrendered to their condition, nor did they ever become a separate outcast section of the population. On the contrary, the pressures exerted by a united local working class succeeded in securing more generous relief for them than was generally available elsewhere.

Of course, none of Macintyre's chosen villages are English, and it might have been worth considering more fully how far their militancy was also colored by local nationalist feelings against policies imposed by what was, to all intents and purposes, a central English government. Further, although the communists emerge as the heroes—in stark contrast to the general impression that they totally missed the boat, especially in the nineteen thirties—it must remain a moot point as to how far their dominance in the Little Moscows was because they were communists and how far due to the fact that most of them were local men who wore other hats, be they those of trade union official or of local councilor.

What Macintyre achieves in this stimulating and lively work is a specific study of an important aspect of working-class life in the period, and one that succeeds entirely in breaking out of the institutional framework into which labor history is usually cast. It is a pity that such a worthwhile book should be offered at such a high price and with proofreading that is shoddy even by modern standards.

KENNETH D. BROWN
Queen's University,
Belfast

BRITON COOPER BUSCH. *Hardinge of Penshurst: A Study in the Old Diplomacy*. (The Conference on British Studies Biography Series, new series, number 1.) Hamden, Conn.: Archon, for the Conference on British Studies and Indiana University at South Bend. 1980. Pp. 381. \$19.50.

Charles Hardinge, first baron Hardinge of Penshurst, had a notable official career in the first quarter of this century. After early posts in the Balkans,

Tehran, St. Petersburg, and the Foreign Office, he briefly became British ambassador to Russia (1904–05) before returning to the Foreign Office as permanent undersecretary (1905–10). He then served as viceroy of India for six years before again becoming permanent undersecretary. His last official post was as ambassador to France. Closely connected with the major diplomatic and political issues of his time, he well merits a biography. Although Hardinge himself wrote two memoirs (*Old Diplomacy* and *My Indian Years*), they naturally do not constitute a critical evaluation of his career. The time is ripe for such a task, because a great deal of work has been done by Zara Steiner and others on the general structure of the Foreign Office in this period.

Although this is Briton Cooper Busch's first biography, his previous books on British policy in the Near East and western Asia show that he has a thorough grasp of the problems with which Hardinge had to deal both in his "European" and "Indian" years. That knowledge is particularly apparent in the discussion of Persia before 1914 and of India during the war. It is refreshing to find a writer on diplomacy discussing Indian internal developments with skill and understanding.

Nevertheless, although the author throws much incidental light on the world of "old diplomacy," as a biographer he is a little disappointing. It is almost as though he has been seduced by his subject; there is an all-pervasive blandness in Busch's assessment. Others might have been a good deal more critical of Hardinge's relationship with King Edward VII or rather more severe on Hardinge's handling of the Mesopotamian crisis during the war. More could have been said about Hardinge's general attitude in the years from 1905 to 1910 toward the Liberal government—indeed toward politics in general. We are repeatedly told that he was not a wealthy man, but the figures of earnings revealed from time to time are not put into comparative perspective.

At his best, however, Busch shows that Hardinge was a formidably urbane operator in the world of "old diplomacy." Busch sees that criticism might be made but is reluctant to make it. It is difficult to feel very enthusiastic about Hardinge's second term as permanent undersecretary or as ambassador to France. Taken together, the bomb attack he suffered in Delhi and the death of his wife undermined Hardinge's resilience and capacity for work more than Busch allows. The intolerant enemy of incompetence showed a discernible loss of administrative capacity.

KEITH ROBBINS
University of Glasgow

This is a very lucid, brief description and analysis of the administration that directed the British total war effort from May 1940 to May 1945. J. M. Lee states that the book evolved from a privately published (1977) study of the machinery of government from 1942 to 1952 and from a forthcoming work on the Colonial Office during the war; but although some of the documents that would have been used in those two monographs are referred to here, this book is primarily based on the many published accounts, although obviously informed by Lee's easy familiarity with the subject. Some knowledge of the main events is assumed—and amply provided for those lacking it in the annotated bibliography—and the book is a series of related essays rather than a chronological account. Following a discussion of the nature of the coalition, there are four chapters on particular main issues: strategy and mobilization; economy and production; reconstruction and social reform; and diplomacy, in which the main attention is devoted to Anglo-American relations. On all of these topics Lee has illuminating comments to make within his narrow compass.

The coalition of Conservatives, Labourites, and Liberals had great advantages in its conduct of the war. It was able to draw on nationalist feelings generated during the phoney war and the period of immediate danger at the beginning of its existence. The preparations of the Chamberlain administration were readily accepted from its successor by a network of local governments and voluntary organizations on the basis of ministerial decrees. With the cooperation of the party leaders and the electoral truce, which had no counterpart in the Dominions and the United States, opposition shrank to the level of a mere irritant, and the government was able to operate largely outside the normal rules of parliamentary practice. About half the M.P.s were in government or the armed services, there were few sittings of Parliament, and secret sessions were easily arranged. On the other hand, without public debate and informed press comment, it was difficult for ministers and civil servants to judge the response to their actions and their probable effects on the vital matter of morale. There was much discussion of preserving the coalition in order to maintain unpopular controls during the transition to peacetime conditions, but there was never any suggestion of continuing the unusual constitutional arrangements after the end of the war. All parties must share the credit for this, but the symbol of the commitment to traditional forms was Winston Churchill, who was at once carrying on his personal dialogue with the nation, conducting his own sometimes inconvenient diplomatic relations with other heads of state, and, as this book amply confirms, keeping a sharp eye on the details of government and administration.

NEVILLE THOMPSON
University of Western Ontario

J. M. LEE. *The Churchill Coalition, 1940–1945*. Hamden, Conn.: Archon Books. 1980. Pp. 192. \$20.00.

D. C. COLEMAN. *Courtaulds: An Economic and Social History*. Volume 3, *Crisis and Change, 1940-1965*. New York: Clarendon Press of Oxford University Press. 1980. Pp. xiii, 345. \$44.00.

In "the Biggest Take-over Bid in Britain's History" (p. 201), ICI tried early in 1962 to acquire Courtaulds, the world's largest prewar manufacturer of rayon. This battle of the giants, which forms the climax of D. C. Coleman's third volume, was the outcome *inter alia* of an erosion in the market for rayon goods and an uneasy partnership between ICI and Courtaulds in British Nylon Spinners after 1940.

The structure of the present volume is simple and effective. The narrative and analysis is divided into three parts. The first covers the war and its aftermath to 1954. In addition to the loss of AVC (dealt with in volume 2), domestic output declined during the war, and retained profits were insufficient to maintain the company's assets. After the war, expansion took the form of an increase in domestic plant capacity and investment in overseas ventures undertaken largely for prestige—except for a wood pulp company in South Africa, which was the group's first step toward vertical integration. Until 1951 all went well as rising prices and productivity produced good profits and yields. But after that the price and sale of rayon, the source of nine-tenths of the group's profits, declined with the growth of world rayon output and competition from other man-made fibers. Part 2, "In Search of a Policy," deals with the remedial strategies followed prior to 1962. In this transition period, much was happening below the surface. The board was transformed by an infusion of scientists and economists from redbrick universities, and these "Players," rather than the public school "Gentlemen" hitherto in control, pursued new policies at a time when the chairman's powers were failing. Investment in research led to the development of Courtelle, an acrylic fiber that proved a winner in the home market by 1960; plants to manufacture synthetic fibers were sold to Communist countries; rayon companies in the United Kingdom were acquired to reduce price competition; and paint and packaging firms were purchased to facilitate diversification. By 1961 a "very different entity" (p. 148) emerged that was less dependent on rayon as a source of profits, and it was against this background that ICI launched its unsuccessful takeover bid. When the battle was over, Courtaulds's ordinaries topped 50/- and "The New Team" (p. 222) had to fulfill its promises to its faithful shareholders. Part 3, "The New Order," describes how this was done by forward integration through takeovers, so that products other than viscose rayon contributed two-thirds of the group's profits; and, to obtain the 38 percent of ordinary stock still held by ICI, Courtaulds relinquished its half interest in BNS.

The attitudes and actions of board members form an integral part of the analysis in this volume, which contains "not so much a view from the board room as a view of the Board" (p. vi). Many of Courtaulds's problems originated in the boardroom. Sir John Hanbury-Williams, the chairman for twelve years, "allowed dignity to masquerade as leadership" (p. 27); Allan Wilson and Frank Kearton, the "pair of prima donnas" who contended for the succession, "lurched from mutual admiration to mutual antagonism, pausing only for occasional periods of mutual comprehension" (p. 141). What this study clearly reveals is the importance of decision making in large corporations, particularly those that, like Courtaulds, had structures that stifled the emergence of new policies.

Not surprisingly, in view of Coleman's concern with the board, themes such as labor relations and textile technology are not explored very fully. But in view of the comparison that is drawn between ICI's production orientation and Courtaulds's market orientation, the group's marketing arrangements perhaps deserved more consideration. Such quibbles, however, are not intended to detract from this outstanding contribution to business history that, by focusing on the performance of board members in a large corporation, moves the entrepreneur back to the center of the stage.

W. G. RIMMER

University of New South Wales

JEAN ROBIN. *Elmdon: Continuity and Change in a North-West Essex Village, 1861-1964*. Foreword by AUDREY RICHARDS. New York: Cambridge University Press. 1980. Pp. xxxiv, 260. \$34.50.

Until well into the nineteenth century most Englishmen lived in what was essentially a rural society, and a romanticized vision of the agrarian past continues to haunt the English consciousness. Studies of rural communities in late nineteenth- and early twentieth-century England might therefore be expected to have a special value in reflecting continuities that could be set against the more familiar national patterns of urbanization and industrialization. In this scholarly and painstaking work Jean Robin has delineated a number of important strands in the experience of one village, Elmdon in Essex, situated about fifty miles northeast of London. As Robin demonstrates, it is the changes rather than the continuities that are most striking, even in an essentially rural settlement. In the century after 1861, the population of the village fell from 520 to 321. In 1861 nearly half the population was fifteen years of age or less, while by 1964 Elmdon was a village with a predominantly elderly or middle-aged population. The village, largely as an outcome of improved communications, became

more involved in the affairs of the wider world. This process brought some advantages (a broadening of horizons and more diversified employment opportunities) and some disadvantages (the loss of many village shops and services); but in either case it meant the end of the comparatively close-knit world of the Victorian farm laborer. Above all, the influence of the squire and of the body of substantial farmers who had such influence in the daily affairs of Elmdon failed to survive into the mid-twentieth century. The overall pattern, therefore, is a not unexpected one of the decline of old ways in the face of the modern world of the internal combustion engine and mass-produced consumer goods; the author is to be congratulated for having portrayed this decline with such detail and sympathy.

Even so, it is striking that the book's main concern is with such concrete, and indeed often quantifiable, matters as immigration patterns, working conditions, and the like. Despite the employment of oral history techniques, there is little attempt to investigate mentalities, belief systems, folklore, or such topics as crime, control, and gossip. These omissions are curious, although it is possible that they will receive greater attention in the forthcoming companion volume, *Kinship at the Core*. Nevertheless, it would have been interesting to have known more about what the squire and the laborer made of the world in which they lived. These reservations, however, should not obscure the contribution that Robin has made in providing an immensely useful account of the history of one community and in thus furnishing further proof of the value of limited village studies to our understanding of English society between the fourteenth and twentieth centuries.

J. A. SHARPE
University of York

MAURICE LEE, JR. *Government by Pen: Scotland under James VI and I*. Urbana: University of Illinois Press. 1980. Pp. xiv, 232. \$16.00.

There is general agreement among Scottish historians on both sides of the Atlantic that James VI of Scotland was a good and successful king of the Scots before he left for England in 1603. Maurice Lee, Jr., does not dispute this; his tremendous knowledge of the history of Scotland prior to James's departure, however, serves him well in his attempt to continue the study of Scottish politics from 1603 to 1625 and to test the accuracy of the famous remark of King James that he could govern Scotland by his pen as he ruled both kingdoms from London.

Lee begins his study of Scottish politics by pointing out that King James began his preparation to

succeed Elizabeth I long before 1603. Part of this preparation was to establish a peaceful and orderly Scotland that could be governed by the Privy Council in Edinburgh. The problem areas were the highlands and the borders, where James had more success than many of his predecessors in establishing law and order. The Kirk, under the influence of Andrew Melville, was a constant source of worry for James until Andrew was exiled in 1606. James had hopes for a complete union of both crowns and parliaments; the English, however, were not in favor of the latter, and James was to die frustrated without achieving complete union.

In his analysis of the administrations of Dunbar and Dunfermline, Lee clearly demonstrates that James gave orders by voice and pen to his capable representatives in Scotland, but many times these orders were amended and sometimes ignored by Dunbar and Dunfermline. In fact, sometimes Dunfermline even suggested alternative courses of action because he was closer to the needs of the Scots than was James. The economic problems of Scotland, especially solvency, were to plague James throughout his reign.

If James ruled Scotland by his pen from 1603 to 1625, either he had a lot of help or his rule was substantially different from what it was prior to 1603. Perhaps it was both. Dunbar and Dunfermline were very different in their methods of governing Scotland with the king's blessing, but both were effective. The Scots as a whole were accepting more and more centralized government from the council in Edinburgh. Also the structure of society was changing, and the social bonds of kinship and lordship were weakening. The average Scot was looking to the parish as a social unit and was enjoying the peace and growing prosperity.

Lee is to be commended for a well-written political history of the period. His treatment of the religious question is lucid and balanced. The documentation is clear and extremely helpful; a bibliography, however, even a limited one, would have enhanced its scholarly presentation. This work will aid the scholar as well as the student of this important period of Scottish history.

CHARLES H. HAWS
Old Dominion University

R. H. CAMPBELL. *The Rise and Fall of Scottish Industry, 1707-1939*. Edinburgh: John Donald Publishers; distributed by Humanities Press, Atlantic Highlands, N.J. 1980. Pp. ix, 208. \$31.25.

The relationship between Britain's nineteenth-century success in "basic industries" and its twentieth-century problems is now a dominant theme in economic history. R. H. Campbell has produced an in-

telligent and well-documented discussion of the problem within the context of Scottish industry. Scotland provides a useful focus for such a study. On one hand, the political structure of the United Kingdom has left Scotland somewhat separate, both culturally and statistically; on the other hand, the basic industries centered in Glasgow dominated Scotland's industrial structure. This study examines the sources of nineteenth-century success, identifies the passing of these bases of success, and examines the response of Scottish industry in the interwar years.

Campbell divides the basic industries of the nineteenth century into two groups: "generalists" and "specialists." The generalist industries included cotton piece goods, coal, and pig iron. Their nineteenth-century growth rested on the existence of cheap, but exhaustible, reserves of coal and iron ore and on Scotland's initial position as a low labor cost region. The specialists—shipbuilders, heavy engineers, and tweed manufacturers—grew in part as a result of inexpensive labor and raw materials, but their late nineteenth-century prosperity rested on labor-force skill and technical leadership. Decline began for the generalists prior to the First World War, as mineral deposits became depleted and Scottish wages rose to English levels. Technological leadership, however, maintained the specialists' competitive position. Their postwar collapse, in Campbell's analysis, occurred because of a shortage of demand. This argument, advanced with special attention to shipbuilding and associated industries, places particular emphasis on the absence of naval orders as a result of the prevailing policy of disarmament. It is the weakest argument in the study. Campbell draws heavily on the records of John Brown and Fairfields—two companies particularly involved in naval and luxury liner contracts—and seems to overemphasize demand factors. Certainly more complete analysis would need to consider at least the world market as a whole, shipbuilding elsewhere, and the value of sterling.

Chapter 7, which examines the response of the specialist industries in the interwar period, is the most interesting in the book. Drawing on documentary evidence, Campbell argues that the prevailing industry response of "waiting to see" was a reasonable position to adopt in an industry, like shipbuilding, that had long been subject to violent fluctuations of output. While hindsight suggests that the economy would have benefited from movement away from its "overcommitment," the failure cannot reasonably be blamed on the industrial leaders who faced the 1930s with a large existing plant. In the event, it is salutary to be reminded that the existence of that plant paid important wartime dividends.

Campbell has written an interesting book that

should be read by those interested in twentieth-century Britain and in the problems of declining regions and industries. Analytically more could usefully be done; nonetheless this history has much of value.

C. KNICK HARLEY
University of Western Ontario

RENÉ TAVENEAU. *Le Catholicisme dans la France classique, 1610-1715*. In two volumes. (Regards sur l'Histoire, numbers 34 and 35.) Paris: Société d'Édition d'Enseignement Supérieur. 1980. Pp. 297; 300-576.

The vigorous revival of French Catholicism in the seventeenth century has long been a favorite subject for Catholic historians. In this new textbook intended for advanced undergraduates in France, René Tavenaux, the eminent historian of the Jansenist movement, presents his version of the oft-recounted story. Perhaps the principal departure from the standard rendition is in the book's organization by theme rather than by period. Various chapters take up the questions of church and state relations, ecclesiastical structures, clerical and pastoral reform movements, intellectual-theological tendencies, liturgical and spiritual developments, and the artistic portrayal of religion. Such an approach is particularly useful in delineating the evolution of and the interconnections between the various spiritual and theological currents of the century. On the other hand, in a textbook of this kind there is no evident didactic justification for the author's decision to place a chapter on Jansenism before his presentation of the "French School of Spirituality" of Bérulle and François de Sales. This curious inversion of chronology may be more revealing of the author's personal religious preferences. Although the spirituality of Bérulle—who is compared to Copernicus—is presented as the high point, in a sense the climax of the century (and of the book), there is much less sympathy for the later development of Jansenism and undisguised contempt for the more emotional religious sensibility, the "mièvrerie" of the end of the century.

Much of the work is conceived as a summary of previously published research. The author does add some interesting insights into possible psychological interpretations of the intensity of seventeenth-century spirituality (an alternative to the Lucien Goldmann thesis, which he rejects) and into the regional development of the French Catholic Reformation along an eastern "Lotharingian Axis" from Savoy to Lorraine. Nevertheless, the interpretive scheme remains profoundly elitist in its orientation, much in the manner of Henri Brémond's *Histoire littéraire du sentiment religieux* (1916-33), which the author fre-

quently cites. Only two brief chapters give any treatment of the religious experiences of the popular classes. The author simply assumes that the masses were responsive to the major reforming efforts of the Catholic leadership. He makes no reference to the ongoing debate over the relation of popular and elite belief systems or to the suggestion that the seventeenth century may have seen a fundamental parting of the ways in the religious cultures of these two groups.

The book concludes with an excellent bibliography of recent French studies (almost nothing in other languages is included). If accepted as a revised version of the traditional Catholic interpretation, it can serve as a helpful initiation to the subject. But it needs to be complemented by one of the recent syntheses on the religious history of France "from below," such as *L'histoire des catholiques en France* (1980), edited by François Lebrun.

TIMOTHY TACKETT
Catholic University of America

levels were the maritime activities of La Rochelle (convincingly compared with those of Bristol) and the determined effort of the government to take over that commerce and establish its own navy.

If La Rochelle was handicapped by internal divisions, lack of vital military connections; and a feeble, ambivalent ideology without a right-of-revolution rationale, the government had its own problems. In fact, some of Parker's most valuable passages, especially in the concluding chapter, explicate internal contradictions in early modern French absolutism.

Parker does not answer all the problems regarding early modern France, a field of study now "in a state of flux and marked by far-reaching disagreements" (p. 1), but his work—*multum in parvo*—is an excellent, provocative introduction. It explores issues that any serious student of the period will have to come to grips with.

DAVIS BITTON
University of Utah

DAVID PARKER. *La Rochelle and the French Monarchy: Conflict and Order in Seventeenth-Century France*. (Royal Historical Society Studies in History, number 19.) London: The Society. 1980. Pp. xiv, 234. \$39.60.

CLAUDE-FRÉDÉRIC LÉVY. *Capitalistes et pouvoir au siècle des lumières*. Volume 3, *La monarchie buissonnière 1718–1723*. New York: Mouton; distributed by Walter de Gruyter, New York. 1980. Pp. 531. DM 80.

The fall of La Rochelle to the royal troops in 1628 is usually presented as an example of Richelieu's successful strengthening of the crown by refusing to accept a state within a state. David Parker, who teaches French history at the University of Leeds, here offers an in-depth re-examination of what he sees as "a microcosm of the tensions in French society" (p. 4). It is not a local history of the type produced in recent years by French demographic and social historians, but Parker has done his homework, relying on other scholars where necessary, but also working through a body of published and unpublished primary sources.

The chapters take up different facets of the complex development: the conflict of allegiances, the government, the economy, the nobility, the religious situation, and ideological groupings. One by one, textbook simplifications are shown to be inadequate. The struggle was not simply between La Rochelle and the king; there was also the question of who should rule at home that divided Rochelais one from another. The religious question was not the simple rejection by Richelieu of the Huguenots' so-called political rights; especially during the 1610s a carrot-and-stick governmental program had steadily weakened the Protestants in a way that anticipated Louis XIV's later measures leading to the revocation of the Edict of Nantes. Particularly crucial was the loss to the Protestants of support from powerful nobles. Meanwhile, operating on other

Either in print or in conversation, various historians have used such words as "eccentric," "confusing," and "fascinating" to describe the first two volumes of this series by Claude-Frédéric Lévy. The same can be said of volume 3. In many ways this book is exasperating. It has no introduction or conclusion, and its pages abruptly jump from one topic to another. Despite its title, the book spends as much time on politics, diplomacy, religion, and personal anecdotes as it does on the subject of financiers and the state during the final years of the regency. Lévy devotes an inordinate amount of space to the diplomatic intrigues of a long list of secondary characters who worked for Sweden, Holland, Spain, England, and the Jacobites. And Lévy's taste for *la petite histoire* often leads him to lamentable excesses: for example, a one-page quotation describing the bladder ailments of Cardinal Dubois in the summer of 1723.

Despite its many faults, however, this volume will prove indispensable for scholars working in the area. Lévy has done extensive work both in French and in numerous foreign archives. He also makes commendable use of several recent works in English. (One note even cites a Marx Brothers movie.) Readers will therefore find that Lévy provides them with a treasure trove of information on both large and small points. For example, the author has consulted a little-known collection of papers in the Rijksarchief in Limburg at Maastricht to shed new light on the question of how much money John

Law was able to send out of France before the collapse of his system. Lévy's conclusion is that Law's financial status during his final years was rather good. Law had managed, for instance, to smuggle a large art collection out of the country.

In several ways Lévy's work can be compared to J. H. Shennan's recent book, *Philippe, Duke of Orléans: Regent of France, 1715-1723* (1979). Both Shennan and Lévy argue for a more positive evaluation of the regency than has been traditional. In the area of foreign relations, each author admires the work of the Abbé Dubois. Despite his personal indelicacies and his comic opera quest for a cardinal's hat, Dubois was able to develop a system of collective security that kept France out of any major wars during this period. Shennan and Lévy differ, however, in their assessment of the duke of Orléans. Whereas Shennan sees the regent as cautious and prudent, Lévy labels him indecisive and vacillating. This is perhaps the reason for the subtitle of the volume: "the truant monarchy."

The many new insights and interesting vignettes that Lévy presents help to compensate for the book's flaws. In short, the parts are greater than the whole.

THOMAS J. SCHAEFER
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MICHAEL P. HANAGAN. *The Logic of Solidarity: Artisans and Industrial Workers in Three French Towns, 1871-1914*. Urbana: University of Illinois Press. 1980. Pp. xv, 261. \$15.00.

The three towns mentioned in the subtitle are Rivede-Gier, Saint Chamond, and Le Chambon-Feuergrolles, all located in the Saint Étienne basin of the upper Loire Valley. Michael P. Hanagan uses each as a distinct case study in order to prove his thesis that artisans, that is, skilled workers, were the effective agents provoking syndicalist and strike activity in the basin. He further posits that the chief motive behind their strikes was not economic gain, because artisans were well paid, but their effort to maintain traditional modes of work organization. These traditional modes, artisanal in character, were not compatible with mechanized manufacture or with extensive managerial control over workers and their techniques. A logical conclusion is that the history of labor in Saint Étienne was the struggle of craftsmen against machines that threatened their skills and managers that threatened their freedom.

That artisans were more capable than unskilled and semiskilled laborers of organizing and striking is a well-established view in much of the monographic literature over at least the last thirty years. Hanagan, after studying these three towns, offers valuable additional evidence in its support. In Rivede-Gier the glassworkers and in Le Chambon the

file cutters were the artisans who provided leadership and initiative to the semiskilled textile, metal, and bolt workers. These latter were incapable of effective union building and sustained strikes, because they were incapable of attaining labor solidarity.

The logic of solidarity lies in the technology of manufacture and the pattern of worker residence. Artisans, who determined their own methods of production, were grouped on the job and created ties with one another during brief pauses on the plant floor. They also lived in close proximity and socialized in neighborhood cafés. The café or barroom served even as headquarters and meeting places for their unions. In addition, artisans with higher salaries had the financial means to sustain both their unions and their strikes and could count on extensive support from federated unions elsewhere in France. Semiskilled workers could not socialize in plants where noisy machines drowned conversation and demanded unrelenting physical motion as well as careful attention. In addition, their housing had no pattern of concentration, being spread widely over the town and without focal points such as neighborhood cafés. Solidarity, therefore, was highly dependent on factors other than human volition, and volition was itself conditioned by the role of technology and housing practices. There is one other variable: sex. Those industries such as textiles and bolt making, in which semiskilled females made up a sizable part of the labor force, were also lacking in solidarity and therefore weak both in strike activity and in the growth of collective consciousness. The logic of solidarity also refers to the efforts of artisans to win the support of semiskilled workers in the defense of their traditional high status and salary in glass making and file cutting.

What remains vague is the ideological basis of this form of solidarity: was it class consciousness or simply professional self-interest? Hanagan states that the latter does not preclude the former but does not develop this issue. Perhaps his main weakness in this matter is due to his relative neglect of political struggles. Social history is not all of history with politics left out. Indeed, to penetrate social consciousness, the historian must study the relation between both social and political cultures, and Hanagan's weakness in this regard leaves some important questions unanswered.

This weakness, however, does not really detract from my enthusiasm for this monograph; it is the result of considerable labor in local archives combined with extensive thought about the role of artisans in changing industries.

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FRANCIS SCHILLER. *Paul Broca: Founder of French Anthropology, Explorer of the Brain*. Berkeley and Los Angeles: University of California Press. 1979. Pp. ix, 350. \$25.00.

Francis Schiller's biography of Paul Broca is the first large-scale study of the work of this versatile French physician who left his mark on several areas of scientific endeavor. Before he died at the relatively early age of fifty-six in 1880, Broca was active in descriptive anatomy, especially brain anatomy, in several areas of general medical pathology, and as a founder and prolific contributor to the emerging science of anthropology. Schiller, himself a physician, exhaustively documents Broca's work in most of these fields with great care, relying on the extensive published medical and scientific literature in which that work appeared. For the specialized student of the history of nineteenth-century medicine or early French anthropology, Schiller has unearthed much interesting material on Broca, his career, and the great number of scientific controversies in which he took part.

This book is very largely a work of medical hagiography, however, and suffers from the various defects of the genre. Many of Broca's contributions are discussed not only in terms of the problems they resolved for Broca's contemporaries but also in relation to modern medical theory and practice. In this sort of medical history, special attention is given to the discoveries of the past that have contributed to current knowledge. Schiller thus spends considerable space on controversies of priority where scientific progress was involved, such as the identification of the famous "speech center" on the frontal lobe of the brain, but spends much less space, or none at all, on those parts of Broca's work that led to scientific dead ends. Another curious feature of Schiller's study is his reproach to recent medical specialists who have failed to acknowledge Broca or one of his contemporaries as the "true" authors of this or that discovery or technique. The fact that much of Broca's work was on cerebral localization and brain function, an area of great current neurological interest, has intensified this ruminative approach to the past.

Other unfruitful aspects of this study are Schiller's habit of quoting huge chunks of Broca's writings where a brief summary would suffice and his extraordinary overvaluation of biographical materials from the period 1841-57. It seems that Broca's published correspondence covers that period alone; thus we are compensated for learning little of the relationships and scientific patronage of Broca's greatest years of power and influence by learning in overwhelming detail about the furnishing of his rooms in his student years and the assorted intrigues of his medical apprenticeship.

Occasionally this approach is useful. The account

of the *concours* process and the medical patronage network of mid-nineteenth-century Paris that emerges from the early biographical material may be one of the best versions in any language. Schiller's spade work on the origin, membership, and organization of the major scientific societies of the period—particularly the Société de Biologie and the Société d'Anthropologie—will also prove to be of great use to students of this period.

The portrait of Paul Broca that results from Schiller's devoted investigation is that of an honest scientist, whose great respect for evidence and whose championing of free scientific inquiry against a host of religious and political opponents were the chief sources of his contemporary celebrity. Many scientists whose scientific reputations are far greater than Broca's have fallen short of the exemplary standards he set for intellectual integrity and decisive evidence in scientific theorizing.

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SERGE BERNSTEIN. *Histoire du Parti Radical*. Volume 1, *La recherche de l'âge d'or, 1919-1926*. Paris: Presses de la Fondation Nationale des Sciences Politiques. 1980. Pp. 486.

Serge Bernstein has made a major contribution to our understanding of the Third Republic. His work now stands along with Claude Willard's *Les guesdistes* and Annie Kriegel's *Aux origines du communisme français* as a model of imaginative and exhaustive research that enriches our understanding of the political process. Indeed, Bernstein's contribution may, because of his subject matter, be the most important.

The Radical party has long been recognized as the pivotal party of the Republic. But Bernstein's work (this is the first of two volumes) is the first comprehensive history based on a thorough study of party and government documents, personal papers, and interviews. The present volume focuses on the years 1919-26, during which the party made a remarkable political comeback, only to fail in government. Bernstein's thesis is that failure was due to the party's return to its prewar strategy and prewar program, which were inadequate in the postwar era. One need not agree with Bernstein's thesis, which is hardly original, to appreciate its careful and lucid documentation. The work is grounded in a solid introduction that deals with the politics leading to the party's founding in 1901 and its formative years through 1914. Starting then with the unfortunate effect of the wartime *union sacrée* on the party, he goes on to describe its organizational revival under Édouard Herriot. He deals, too, with the party's gradual movement to parliamentary opposition under pressure from its revitalized organi-

zation, the formation of the successful electoral alliance of 1924, and the unsuccessful governing experience of 1924–26.

The book's most valuable section, however, is the description of the party during the years 1922–38. It is a careful and detailed account of the party's structure and a remarkable account of the activists who gave life to that structure. Bernstein is to be commended for his imaginative use of the executive committee lists published at the end of the party's annual congress reports. The failure of the party to produce precise figures on party membership in the Socialist and Communist manner has often been cited as evidence of the party's organizational inferiority. Yet Bernstein uses the Radical executive committee lists to describe a level of political activism that membership figures alone do not reveal. The Radical lists name names and give places of residence. They often list too the political offices held by the delegates and their occupations. Where they do not, Bernstein has tracked the information down to provide us with a picture of the Radical militants that simply does not exist for their Socialist and Communist counterparts. This type of analysis makes Bernstein's work invaluable not only for students of the Third Republic but also for students of comparative politics.

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ANDRZEJ PACZKOWSKI. *Prasa i społeczność polska we Francji w latach 1920–1940* [The Press and Polish Society in France, 1920–40]. Summary in French. (Biblioteka Polonijna, number 5.) Wrocław: Ossolineum. 1979. Pp. 243. 60 Zł.

This most recent effort by Andrzej Paczkowski concerning the history of the Polish press in the late nineteenth and early twentieth centuries focuses on the Polish emigrant press in France in the years from 1920 to 1940 and on its political, social, and cultural implications for the approximately one-half million Poles who settled in France during that period, chiefly for economic reasons. The monograph covers relatively well the subject indicated by its title, despite the very limited source materials utilized by the author. The three major chapters, each of which contains several subheadings, concentrate on the nature of Polish society in France, the emigrant press, and the press in emigrant society. A brief introduction precedes the body of the text, and a succinct conclusion follows it. A useful summary in French is appended. Although footnotes appear throughout the text, the study lacks a bibliography. The inclusion of two separate lists of surnames and of titles of Polish newspapers circulating or published in France during this period in lieu of

a comprehensive, integrated index detracts from the study.

Chapters 1 and 2 are carefully documented and informative, and they read well. Culturally separated from the French and distinctly different from earlier Polish settlers in France, the predominantly rural-urban working-class Poles on whom the author concentrates reflected a culture unique to their ethnic and social backgrounds. The Polish press in France, Paczkowski suggests, served their diverse needs in diverse political and apolitical ways. The author introduces the reader to the large number of Polish publications available in France in the inter-war period. He discusses adequately the most influential ones, *Wiarus Polski* (Lille) and *Narodowiec* (Lens), which originated in Westphalia. He then identifies and in intricate detail describes the vacillating fortunes of the numerous, less influential, and ephemeral publications, for example, *Świat Polski* (Paris, December 1938–May 1939) and various Polish Communist publications (sixty-one, according to his index).

The final chapter displays some flaws. Most notably, Paczkowski repeatedly refers to the implicit influence of a collective Polish Communist press in France without either identifying singular specific Communist publications in his text or documenting his statements (pp. 183, 191, 193, 199, 205). Another flaw inheres in this brief third chapter, which the author subdivided into seven subtopics. Each of the subtopics is dealt with rather superficially. For example, "Czytelnictwo prasy" ("Press Readership"), largely a hypothetical discourse, lacks substance.

Despite these flaws, the monograph emerges as an informative, coherent, and readable study, but the subject matter is of peripheral interest except to the specialist.

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PHILIP G. CERNY. *The Politics of Grandeur: Ideological Aspects of de Gaulle's Foreign Policy*. New York: Cambridge University Press. 1980. Pp. xi, 319. \$29.50.

The subtitle of this book is *Ideological Aspects of de Gaulle's Foreign Policy*. Indeed, its purpose is not to study the general's diplomacy but to show how he used foreign policy as the best way to rebuild a national consensus, to revive and strengthen national consciousness, and to legitimize "a new structure of authority" (p. x). Since foreign policy in France is "the only reliable basis for building . . . a symbolic consensus" (p. 88), a new state linked to national identity in the public consciousness would be a strong and legitimate state. Grandeur is thus seen not as an anachronistic dream of hegemony nor as an ambition of "glory and power for France for its

own sake" (p. 6)—and well beyond its means—but as the expression of "the need to create a new and more profound sense of national consciousness, capable of transcending . . . traditional divisions" (p. 14). In other words, in Philip G. Cerny's view, achieving external results (such as succeeding in undermining the preponderance of the superpowers and dismantling the military blocs and in establishing a new international system in which France would be one of the great powers and could also appear as a kind of mediator between the new or small nations, to whom de Gaulle appealed, and the other great powers) was less important than succeeding in modifying "certain salient characteristics of the belief system of French mass and elite publics and in channeling beliefs in such a way that they should reinforce public compliance with the norms of the Fifth Republic itself" (pp. 257–58).

Cerny provides one of the subtlest analyses of de Gaulle's conception of the nation, of international affairs, and of political leadership (he rightly stresses the moral ingredients of de Gaulle's view of politics). His remarks on charisma as an ability "to impose rules of its own" (p. 119) and to reduce individual alienation are shrewd. His discussion of de Gaulle's foreign policy design and of the crisis of the Atlantic alliance provoked by the General—which serves as the book's detailed case study—is equally well informed and intelligent. He shows how foreign policy helped de Gaulle cross the left-right divide and maintain "the crucial symbolic links between presidential and legislative Gaullism" (p. 259).

Two criticisms may be addressed to this work. First, with respect to its style, it suffers, at times, from an overdose of the kind of jargon that often afflicts discussions of symbols, images, myths, and communications; many passages could have been put in far plainer English (and Cerny writes most elegantly whenever he does not feel the need to give us a lesson in political symbolism). Second, concerning the substance, he is right in pointing out that de Gaulle was less concerned with external results than with making the French "believe that France once again had a significant role to play in the world" (p. 90). But he is wrong in assuming, as he does throughout the book, that the restoration of consensus, national pride, and public authority was the supreme goal. It was both a very major goal and a means; although foreign policy was de Gaulle's chosen instrument for rebuilding a state and reviving national sentiment, both the nation and the state had a vital role to play on the world scene. Cerny himself stresses the teleological nature of de Gaulle's idea of leadership. France needed "great enterprises," not only to overcome ferments of division but also because this is, quite literally, the purpose of life, and France needed a strong state to

carry out such tasks. What the enterprise would be depended on the circumstances, and the fight mattered more than full victory; but initiative and action were constant imperatives. This is precisely why, in the *Edge of the Sword*, de Gaulle had quoted Goethe's Faust: "in the beginning was the deed."

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JEAN STENGERS. *Léopold III et le gouvernement: Les deux politiques belges de 1940*. (Document Duculot.) Paris: Éditions Duculot. 1980. Pp. 248.

This book is set in the eighteen chaotic days that began with the Nazi invasion of France and the Low Countries on May 10, 1940, and culminated in the Belgian military capitulation. In it Jean Stengers tries to relate how disagreements over policy toward the enemy led the Pierlot cabinet to flee the occupied country to exile in London while King Léopold remained behind.

Earlier authors (Jules Gérard-Libois and José Gotovitch, Albert de Jonghe and Fernand Vanlangenhove) agree that after the initial German victories Léopold was resolutely defeatist and the cabinet divided. In their view, the different destinies of the two owed as much to circumstance as to disagreements over policy. *Léopold III et le gouvernement* offers nothing original in the way of evidence or argument either to challenge or substantiate these conclusions.

The underlying purpose of this study was to shed light on the post-World War II "Royal Question," which for a time threatened to plunge Belgium into civil war and was settled only with the eventual abdication of the monarch. The struggle pitted those who held Léopold responsible for having led the country into a policy of collaboration with the Nazis against those who had been too compromised by it to admit publicly that it had been in error. A search for the origins of this crisis might well have led to the first major Belgian study of Belgian society during the occupation rather than to a recrossing of terrain long familiar to historians and the Belgian public alike.

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JENS E. OLESEN. *Rigsråd—Kongemagt—Union: Studier over det danske rigsråd og den nordiske kongemagts politik, 1434–1449* [Rigsråd—Royal Power—Union: Studies on the Danish Rigsråd and Royal Power Politics in Scandinavia, 1434–49]. Summary in German. (Skrifter Udgivet af Jysk Selskab for Historie, number 36.) Aarhus: Universitetsforlaget. 1980. Pp. xvii, 620. 216.45 KR.

In the years following 1434, Erik of Pomerania, union king of Denmark, Norway, and Sweden, lost de facto and then de jure possession of his realms. In his place the three councils separately elected his nephew Kristoffer of Bavaria, whose sudden death in 1448 meant that the process of choice had to begin again.

Jens E. Olesen offers a detailed narrative and analysis of this epoch in Scandinavian history. The detail and the way that it is marshaled make his work valuable. That the story is told from the standpoint of the Danish council further enhances its value. Although the Swedish revolt of Engelbrekt in 1434 is the best-known event of the period, Olesen shows that the nobles in each kingdom had their distinctive interests. Danish nobles would therefore support Erik while he was successfully advancing Danish interests within the union. That support was withdrawn when Erik put his dynastic interests above theirs and above the interests of stability.

And if Kristoffer, at least in Denmark, became a more powerful king than is sometimes supposed and indeed strengthened himself vis-à-vis his council, this was partly because he enjoyed good relations with it. Only recently have historians cast off the spell of the vivid, Swedish, and blatantly partisan Karl Chronicle, written to glorify Kristoffer's successor in Sweden. Kristoffer was a more positive figure than the devilish king whose horses ate five lasts of corn while his subjects ate bark bread. Olesen adds further evidence from the Danish side toward his rehabilitation. Like Kristoffer, several of the prelates and lay noble families—the Gyldensterne, Laxmand, and Thott families, for instance—had interests in the Baltic trade. This cooperation between a Danish king and Danish nobles, both using Danish leadership of the union in a bid for Baltic hegemony, was equally a feature of the next reign, that of Christian I.

By giving a group portrait of the Danish magnates, Olesen is able to provide them with an intelligible motivation and to embody this in a fascinating study of policy making and negotiation. What in older books often seemed a dismal and confusing factionalism falls into a satisfying historical shape.

The socioeconomic interests of the noble caste are an important consideration in Scandinavia as elsewhere in early modern Europe. But, as Olesen shows, they could express themselves in terms of a national policy and cooperate with the aims of a national monarchy. This subtle balance of forces in its Scandinavian variant affords a useful parallel for students of other European regions. Olesen's ten-page translation of his conclusions into German should facilitate this comparison. His documentation of his sources and his apparatus, including bio-

graphical data on council members, are lavish and thorough.

JOHN SIMPSON
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JAN STATIN. *Hushållningssällskapen och agrarsamhällets förändring: Utveckling och verksamhet under 1800-talets första hälft* [Economic Societies and Changes in the Agrarian Society during the First Half of the Nineteenth Century]. Summary in English. (Studia Historica Upsaliensia, number 113.) Uppsala: Textgruppen i Uppsala; distributed by Almqvist and Wiksell International, Stockholm. 1980. Pp. 244.

Agricultural societies became widespread in Europe by the late eighteenth century, inspired by French physiocracy, German cameralism, and English agronomy. In Sweden local societies began to appear through private initiative by 1791. The *Riksdag* of 1809 considered a motion for a network of such societies throughout the country, which, while improving agriculture, would promote decentralized self-rule at the county (*län*) level between the central administration and the old, largely peasant, parish councils. In 1811 the new crown prince, Karl Johan—the former Marshal Bernadotte, now de facto regent—seized the initiative by establishing the Royal Agricultural Academy, followed in 1813 by economic societies in every county. Anxious to strengthen central authority, counteract liberal tendencies in the *Riksdag*, and prevent social unrest through lack of provisionment, he made the academy a semi-official department of government, largely corresponding to earlier mercantilistic boards for trade, mining, and manufactures. The economic societies, chaired and partly appointed by the provincial governors, likewise gave Stockholm increased control over county affairs through the unpaid efforts of local notables.

Jan Statin studies seven representative county societies in detail. Although officially private associations with only modest government financial backing, the economic societies performed with varying success a variety of useful tasks relating to agriculture, broadly defined. In so doing, they could not but concern themselves with certain social issues with far-reaching political implications. Nonetheless, faced by mid-century with growing liberal pressures for representational reform of the *Riksdag* and elective county councils, the government came to welcome an expanded role for the economic societies as a conservative alternative.

Although recruited without reference to estate (*stånd*) and thus open to propertied elements still unrepresented in the *Riksdag*, the societies at first gained little cooperation from the peasantry, who saw them as representative of upper- and middle-

class interests in an expansive market economy that were threatening to traditional subsistence farming. By mid-century, however, such opposition weakened as prosperous peasant farmers were increasingly drawn into the activities of the societies through Sweden's ongoing economic transformation.

Concentrating upon the economic societies' political and social significance, Stattin's dissertation provides an illuminating study of the development of local involvement and competency in public affairs preceding the parliamentary and county administrative reforms of the 1860s. One only regrets that it unaccountably stops short, around 1850, and fails even to mention the societies' (and academy's) continued useful existence to the present day.

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OSMO JUSSILA. *Nationalismi ja vallankumous venäläis-suomalaisissa suhteissa, 1899–1914* [Nationalism and Revolution in Russian-Finnish Relations, 1899–1914]. Summary in English. (Historiallisia Tutkimuksia, number 110.) Helsinki: Suomen Historiallinen Seura. 1979. Pp. 325.

The extent to which each generation succeeds in rewriting the essence of its own past depends most particularly upon the ability and daring of its avant-garde scholars. In that respect, contemporary Finland is well served by Osmo Jussila, a historian who relishes his role as revisionist. For example, in his first major publication (*Suomen perustuslait venäläisten ja suomalaisten tulkintojen mukaan, 1808–1863* [1969]) Jussila challenged the notion that the first half-century of Russian rule had passed without conflict over Finland's special status in the empire. Now, in *Nationalismi ja vallankumous*, he reverses tables by trying to establish the lines of cooperation and collaboration between Finns and Russians during the height of their conflict over the issue of Finnish autonomy. Although Jussila's revisionism may seem no more than a highlighting of the contrary tendencies in Russo-Finnish relations, his methodology derives from the principles of the Hegelian dialectic and modern science.

Jussila outlines the history of the two periods of Russian "oppression" in Finland. The first began with Nicholas II's proclamation of a manifesto in February 1899. By establishing the principle that imperial legislation would take precedence over local Finnish laws, customs, and privileges in matters of common concern to the empire, the February manifesto enabled the Russian government to pass an act to conscript Finns into the imperial forces. When the tsar failed to heed their initial protests,

the aggrieved Finns adopted the tactics of passive and active resistance. This radicalization of Finnish discontent, however, did not achieve notable success until the outbreak of the Russian revolution of 1905 and its subsequent spread into Finland. Temporarily paralyzed by a general strike, the Russian government was forced to end its "oppression" by making concessions to Finnish nationalists and revolutionists. By 1907 the Russian government had regained its confidence and was prepared to restore order to this part of the empire by instituting a second period of "oppression." Although its autonomy was threatened by a law passed by the Russian Duma in June 1910, the outbreak of the First World War spared Finland from its extinguishment under new legislation drafted in 1914.

Notwithstanding the severity of those tensions existing between Finland and Russia, various factions within the Finnish opposition also found it expedient to cooperate on occasion with the Russian government. Indeed, Jussila's attention is primarily focused on those Finns who "traveled the road to St. Petersburg." Although he recalls the course of the compliers and constitutionalists who followed this path with the hope of eliciting imperial support for their own political advantage as well as on behalf of Finland, Jussila reserves his particular interest for demolishing the later contention of the Social Democrats that the "bourgeoisie" should be blamed for taking that road just for the sake of their class interests. In this regard, Jussila notes how certain Social Democrats (Valpas-Hänninen and others) were equally ready to sacrifice the cause of autonomous Finland for the sake of the class struggle. Thus, he concludes that the Social Democrats were by no means "blameless" in weakening the Finnish resistance to Russian authority.

Although some of this could strike the non-Finn as too much an exercise in partisan politics, *Nationalismi ja vallankumous* is an important contemporary statement on that part of the Finnish past as well as on "the state of the art" in Finnish historiography.

EDWARD W. LAINE
Public Archives of Canada

ERWEIN H. ELTZ. *Die Modernisierung einer Standesherrschaft: Karl Egon III. und das Haus Fürstenberg in den Jahren nach 1848/49*. Sigmaringen: Jan Thorbecke Verlag. 1980. Pp. 268. DM 38.

The German *Standesherren*, a peculiar social class resulting from the reorganization of Germany under Napoleon, were so novel that a new term had to be created for them in order to write the necessary legislation to describe their unique, semisovereign privileges. After 1918 this anomalous class of approximately eighty families lost its constitutional

status. What other country had a social class created by law that lasted only a century? Relics from Germany's feudal past, these "mediatized" petty rulers struggled to survive in a world of nation states and a capitalist property system.

Erwein H. Eltz's *Die Modernisierung einer Standesherrschaft* deals with one of the most prominent of these families, the Fürstenbergs of the Black Forest, and especially the prince, Karl Egon III. Eltz's theme, as declared in the title and introduction, is the modernization of the Fürstenberg's holdings into an economically profitable estate and a cultural force in southern Germany. Theories of modernization, however, play only an incidental role in the book. This is unfortunate, since the Fürstenberg family had an interesting history and this book, if written from a comparative perspective and explicitly engaging modernization theory, could have contributed more directly to larger issues in German history.

The book, published with the aid of the present Fürst Joachim zu Fürstenberg, is still worthwhile. Karl Egon III (1820–92) succeeded in transforming his lands and feudal rights into an enlarged, private estate after his accession in 1854. He hired a manager, Johann Prestinari, to put the estates on a profitable, capitalist basis. Prestinari changed former civil servants into employees, phased out uneconomic activities, and expanded, rationalized, and set the family's forests on a moneymaking path. The prince also hoped to maintain family influence by establishing a cultural role for his family and, especially, to help South German Catholicism overcome its intellectual isolation and backwardness. A reorganized archive, a working natural history museum, and an extensive library were primary means to this end. These and other cultural activities continue to this day to keep Donaueschingen, their capital, on the map. Karl Egon III also dabbled, reluctantly and somewhat unsuccessfully, in politics (he lost an election to his own court apothecary) and headed the German union of *Standesherren* from 1863 to his death. All of these topics are well developed in Eltz's book, each being based on a thorough evaluation of archival materials. Aside from Heinz Gollwitzer, *Die Standesherren* (first published in 1957) and a few other studies, the *Standesherren* have received little attention, although they influenced many developments in Germany during the century. This book adds a fund of information and detail to make the achievements of one of their number better known.

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New Paltz

HANS DIETER DENK. *Die christliche Arbeiterbewegung in Bayern bis zum Ersten Weltkrieg*. (Veröffentlichungen

der Kommission für Zeitgeschichte, Forschungen, number 29.) Mainz: Matthias-Grünwald-Verlag. 1980. Pp. lxxix, 429. DM 88.

Writing on the history of the German labor movement has traditionally had a marked organizational emphasis. So has the literature on the social and political movements of Catholic Germany. The present work, which deals principally with the Catholic workers' movement in Bavaria, shares this pedigree and this approach. Against more recent patterns of research on German labor that have stressed shop floor experience, geographical mobility, and the popular culture of everyday life, Hans Dieter Denk makes organization the backbone of his account. We learn what stimulated it (industrialization, the socialist challenge), how it grew (locally and regionally, administratively, and in its educational functions), and what obstacles stood in the way of it (especially the demarcation disputes between the different wings of the movement).

The result is a detailed and scholarly account of the various stages and institutions through which Catholic workers were organized in Bavaria before 1914 (the flimsier Protestant organizations are granted the space they deserve, but no more). The workers' associations of the 1890s developed out of the earlier journeymen's associations and the paternalist, clergy- or bourgeois-led workers' benefit organizations. The workers' associations in turn began to assume more specifically occupational form and were also rivaled from the turn of the century by the emergent (nondenominational but largely Catholic) Christian trade unions. The author's tone in presenting this is unhagiographical, but clearly sympathetic—he refers to the development of the Catholic workers' movement in Eichstätt, for example, as "thoroughly satisfactory" (p. 119).

Among the many strengths of this study is its treatment of social Catholic ideas, which have been the frequent object of blandly uncritical exegesis. The author's account is restrained and plausibly differentiated. The crucial importance of the 1890s for the developments under discussion is also properly brought out. So too is the importance of the lower clergy in the machinery of the workers' associations, although the author also shows clearly the negative side of clerical willfulness—obstructing occupational associations and creating suspicion of the Christian trade unions, stifling lay energies with paternalism. The sections of the book dealing with supraregional organization also helpfully reinforce what we know from other areas of Catholic social and political organization: that there existed, broadly, a reactionary east, progressive west, and parochial south. Divisions in the Catholic Center party ran along similar lines. The overall picture that emerges is one of a significant but divided movement that did not rival the SPD-oriented free

trade unions, a movement, moreover, that almost certainly gave more to the Center party than it received in return. It is perhaps a pity, in this respect, that the sections dealing with the relations of workers' movement and party are scattered throughout the book, and the section that brings the relationship into focus is very short.

This is nevertheless a valuable book, which uses the most recent secondary literature on pre-1914 Bavaria and draws on an impressive array of contemporary newspaper and pamphlet literature in addition to other unpublished sources. A useful index contains references to places and subjects as well as names.

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DAVID BLACKBOURN. *Class, Religion, and Local Politics in Wilhelmine Germany: The Centre Party in Württemberg before 1914*. New Haven: Yale University Press. 1980. Pp. xv, 267. \$25.00.

Recent historiography about imperial Germany has been dominated by the interpretations of a new generation of German historians led by Hans-Ulrich Wehler. Like the essays (including one by David Blackbourn himself) edited by Richard J. Evans in *Society and Politics in Wilhelmine Germany* (1978) and Geoff Eley's *Reshaping the German Right* (1980), *Class, Religion, and Local Politics* is part of a concerted and comprehensive effort by several young British historians to revise and expand the analytical framework fashioned by this new German school.

Blackbourn argues that the focus of his study—the Catholic Center party in Württemberg—does not fit most of the generalizations made by Wehler and his associates. Central to the new German interpretation, of course, is the conviction that the survival of authoritarianism in imperial Germany must be explained in terms of the manipulative abilities of an agrarian-industrial alliance at the top of society to defend its position against the forces of democratization. This explanation, however, is challenged by Blackbourn's basic premise that reformist tendencies within the Center were frustrated by pressures welling up from the Centrist political constituency itself.

Denying that religion was one of these pressures, Blackbourn instead locates the causes for the Center's customary progovernmental and antileft stance during the Wilhelmine era in the backward economic conditions characteristic of the Roman Catholic districts. This "backwardness," which he skillfully and sensitively describes, led the Center to assume political positions that estranged it from the

reform-minded parties on the left. On the one hand, a new generation of bourgeois Centrist leaders, animated by the desire to achieve equality and respectability, cut themselves off from the left by a display of nationalistic fervor. On the other hand, economic retardation meant that the peasantry and a *Mittelstand* composed of artisans and shopkeepers were overrepresented among the Center's grassroots political supporters. Pressured from below by this constituency, the party was compelled to espouse protectionist policies that further divided the Center from the Social Democrats and the Progressives.

But agrarianism and *Mittelstandspolitik*, Blackbourn maintains, could not satisfy all the disparate factions within the Center. Faced with the task of harmonizing these contradictory interests and stemming the defection of the Catholic working classes, the party leadership resorted to a demagogic political style, Blackbourn claims. Although this approach contributed to a superficial unity, it also led the Center to adopt slogans indistinguishable from those of the imperial government and the Conservative party, contributed to the forging of closer ties with the right, and inevitably discredited the Center with the left.

For all his ability to unravel the complexities of class interest and economic grievance, however, Blackbourn remains unable to integrate the confessional factor into his analysis. A major reason for this situation is his systematic avoidance of the ecclesiastical side of the written record. His reluctance to treat these matters leaves unexplained the reasons, if the religious appeal was insignificant, why the Catholic peasantry and *Mittelstand* did not in the end abandon the Center for the Conservative party. Nor for that matter is a discussion of the Center's demagogic style complete without a consideration of the sectarian component.

Blackbourn's findings, interesting and useful though they are, are not the result of an innovative methodology. They are instead the consequence of a change of focus—a shift from a concern with the traditional ruling elites to a treatment of the Centrist leadership and rank and file. Although this approach unfortunately does not provide convincingly hard quantitative data about the composition of the social groups that are the object of Blackbourn's attention, it does suggest the need to revise some of the historiographical conventions of the new German school. The necessity to expand the German analytical framework to take account of "pressures from below," however, should not obscure the shared assumption of both Blackbourn and Wehler that the forces working for social and political emancipation in Wilhelmine Germany were ultimately frustrated by an unmodern social structure.

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GERHARD WAGNER. *Deutschland und der polnisch-sowjetische Krieg 1920*. (Veröffentlichungen des Instituts für Europäische Geschichte Mainz, number 93.) Wiesbaden: Franz Steiner Verlag. 1979. Pp. viii, 294. DM 74.

This is an important and well-documented monograph illuminating German foreign policy and internal politics shortly after the birth of the Weimar Republic. Gerhard Wagner's aim is in part to "demythologize" the Polish-Soviet war. In the first chapter, Wagner's incisive summary of the conflict gives it more modest strategic and political dimensions than have some historians. Indeed his account helps put to rest once and for all the view that a Bolshevik tide was about to sweep over Europe in 1920 but was "miraculously" turned back on the Vistula when the Polish army successfully counter-attacked in August.

The remaining chapters concentrate on German perspectives, aims, and actions. Military and political leaders in Berlin and East Prussia agreed that German military intervention was out of the question and neutrality the only practical policy. Yet they hoped the Red Army would conquer Poland and interpreted their declaration of neutrality (July 20) so as to favor the Soviets. (A notable exception was the German ambassador in Warsaw, Count Alfred von Oberndorff, who considered Poland an essential buffer against Bolshevism.) The Germans discussed with Soviet representatives the restoration of the borders of 1914 and assumed that Britain and France would grant Germany economic and military concessions at the Spa conference out of fear of the Bolshevik threat. But the two major premises of German policy proved false. Soviet forces were not strong enough to conquer Poland, and Western pressure on Germany to fulfill the terms of the Versailles treaty did not significantly decrease as Soviet troops advanced.

Wagner's major contribution is his examination of repercussions in East Prussia. There the many threads of foreign policy and internal politics were in closest proximity to the war itself. Drawing from unpublished documents in the archives of Potsdam and Merseburg as well as Bonn and Koblenz, Wagner shows the complexity of maintaining neutrality along the borders of East Prussia. Berlin's instructions to East Prussian officials warned against either violating or recognizing the borders established at Versailles. German authorities dealt with border violations by retreating Polish and Soviet troops and dampened the possibility of outbursts by paramilitary organizations in East Prussia or by Germans living in the territories recently lost to Poland.

After Poland won the war, German leaders began to recognize that chances for territorial revision in the East lay not in "spectacular conflicts" but in a

long-term and carefully constructed policy. Wagner might have expanded this conclusion to place the episode of 1920 in the larger context of Weimar foreign policy over the coming decade, for the idea of using the Soviet Union as a counterweight against Poland and the West survived the Polish-Soviet war. Still, this is a valuable book, its argument clear and reasonable.

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SANFORD RAGINS. *Jewish Responses to Anti-Semitism in Germany, 1870-1914: A Study in the History of Ideas*. (Alumni Series of the Hebrew Union College Press.) Cincinnati: Hebrew Union College Press. 1980. Pp. xiii, 226. \$17.50.

With the appearance of Ismar Schorsch's *Jewish Reactions to German Anti-Semitism, 1870-1914* (1972), a new period of scholarship on German Jews had begun. Reacting to the twin, post-Holocaust critical obsessions with historical German antisemitism and the self-denying assimilationism of German Jews, Schorsch, Jehuda Reinharz, Stephen Poppel, Majorie Lamberti, and others focused instead on the positive German-Jewish institutional responses to antisemitism. They found that German Jews achieved a Jewish self-awareness and a sense of Jewish solidarity through self-defense and Zionist mobilization. So powerful is this swing from the earlier, negative evaluations of German Jewry that other writers, most recently Frederic Grunfeld, see a positive Jewish spirit infusing all German-Jewish life.

Sanford Ragins enters this controversy over the degree of Jewish distinctiveness with a sober and well-written account of both the institutional and cultural Jewish encounters with German antisemitism. After examining the adhesion of German Jews in the nineteenth century to the liberal ideals underlying their emancipation, which promised to bring Jews from the periphery to the center of German society, Ragins identifies two disparate responses to the antisemitic disturbance of the liberal ideological consensus: an assimilationist rationalization of the antisemitic threat as un-German and temporary and a "postassimilatory" perception of a widening gap between Jews and non-Jews. He concludes that the latter, radical Zionist perspective constituted an unprecedented, antiliberal defection from emancipation ideology and the genesis of a new ideology of Jewish separatism.

Unfortunately, Ragins's conclusion does not follow from the evidence even in his own work of a constant Jewish longing for social integration. As he shows, the radical Zionist self-conception of the

Jewish *Volk* imitated the external forms of German nationalism and intimated their legitimate ties with the general culture. And again, youthful Jews aimed their pursuit of a vigorous physical culture in part at regaining the respect of Germans. How complete could the rejection of assimilation be, if Jews set their militant themes to the tune of "O Deutschland, hoch in Ehren"? The renewed expressions of Jewish nationalism disclose not a defection from the liberal ideology of consensus but rather new strategies for integration.

This is not to say that a Jewish subculture did not exist, or was, at most, a mirage. But, beyond giving examples of a simple Jewish national awareness, Ragins never explicitly develops, for instance, the peculiarly Jewish idealized conception of Germany as a *Kultur*—as a neutral order of common humanity or as a pluralistic order of social co-existence. Moreover, by underestimating the exceptional impact of immigrant, East European Jews on native German Jews, he does not fully make clear the pathological intensity of the latter proving their German or human worth, or the radicalization of German Zionism.

If Ragins's study caps the new scholarship of the past decade on German-Jewish history, it does not advance the recent efforts at defining the discrepant German and Jewish features of Jewish life in Wilhelmine Germany.

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DONALD L. NIEWYK. *The Jews in Weimar Germany*. Baton Rouge: Louisiana State University Press. 1980. Pp. viii, 229. \$17.50.

To the Nazis, the Jews of Germany operated as one cell in a monolithic Jewish world conspiracy. In truth they were extraordinarily diverse in religious orientation, politics, and economic power and as likely to struggle with one another as with the anti-semitic enemy. Donald L. Niewyk examines all the now familiar aspects of their problems. While raising the still hot issues of Jewish cultural domination, Zionism's debt to *völkisch* ideology, and the antagonism of German toward East European immigrant Jews, Niewyk resolutely avoids dramatic or provocative conclusions. Balance is the hallmark of this book, apparent in its treatment of individual problems as well as its overall structure.

After introductory remarks on the history of German-Jewish relations, Niewyk devotes a chapter to Jewish roles in Weimar cultural, economic, and political life and another to the subject of anti-semitism. These early chapters introduce the book's

main theme, the Jewish response to antisemitism. In the Weimar era the Central Association of German Citizens of Jewish Faith (*Centralverein*), embodying a set of values that had evolved over the previous century, led the fight against antisemitism. Patriotically German, politically moderate, thoroughly acculturated, and doggedly assimilationist, the *Centralverein* used methods of self-defense acceptable to the majority of German Jews—legal pursuit of anti-semitic demagoguery, financial support for parties willing to fight antisemitism, and massive publication of counter-antisemitic apologetics. By fighting back, the organization developed an identity for German Jews, one that a majority of them could live with, more or less comfortably. Although the *Centralverein* came closest to speaking for Weimar Jews, its policies and version of German-Jewish identity were attacked by various fringe movements within and outside the organization. Zionism rejected the notion of German nationality for Jews, insisting on primary allegiance to the Jewish *Volk*. The chauvinists of the League of German Nationalist Jews condemned the divided loyalties of the *Centralverein*, claiming that antisemitism was the predictable result of incomplete assimilation into the German nation. Although neither group had significant membership, Niewyk discusses them thoroughly, arguing that they represented the polar extremes against which the majority position defined itself.

That position is under attack even today, as though it, rather than the Nazis, bore major responsibility for the Jewish catastrophe. The unthinking maintenance of traditional ties to a bankrupt liberalism, the self-deluding belief in real assimilation, putting class interests before those of survival and thus refusing to give wholehearted support to communists or socialists, and, generally, the unwillingness to risk offending the sensibilities of the German majority—all these charges have figured in recent criticism of the *Centralverein*. Niewyk sensibly and sensitively defends the association's values and methods. They were reasonable choices for the times and consciously made, not the motor responses of pathetically self-deceived men. The critical distance achieved in this book—its most valuable contribution to the literature—is nowhere more evident or apt than in its summing up: "It is hard to escape the conclusion that there was nothing more that the small minority could have done to save itself. Even if it had been perfectly united, scrupulously free from 'unpatriotic' affiliations, and ready to a man to put money and brains to work against the Nazi foe, it could not have altered the outcome in any significant way" (p. 199).

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WAYNE C. THOMPSON. *In the Eye of the Storm: Kurt Riezler and the Crises of Modern Germany*. Iowa City: University of Iowa Press. 1980. Pp. xi, 301. \$17.95.

Although revolutionary eras such as the early twentieth century are frequently explained in terms of masses and forces, they become accessible only when translated into individual terms; such is the rationale for biography. This genre has particular utility when its subject was not only a participant in crucial events but also a perceptive observer and reflective thinker, as is the case with Wayne C. Thompson's extensively researched, crisply written, and beautifully edited study of Kurt Riezler.

Riezler's career was meteoric, brilliant but brief, lasting from 1906 when at the age of twenty-four he entered the German Foreign Office press section until he resigned in 1920 at the age of thirty-eight as President Ebert's adviser. After 1909 he became confidant and ideas man for Bethmann Hollweg and probably reinforced the chancellor's policy of restraining imperialism, limiting naval expansion, pacifying England, dividing the entente, and avoiding war. Riezler's diary for the July crisis of 1914 reveals the mood—particularly Bethmann's—of desperate fatalism that seemed to demand a calculated risk of war to preserve Austria-Hungary and divide the entente before it overwhelmed Germany. As Bethmann's wartime adviser on war aims, Riezler drafted the expansionist memorandum of September 1914 that sparked the recent controversy over the extent and continuity of German aspirations. The relatively moderate aims of Bethmann and Riezler are generally adjudged by historians as less reprehensible than those of the extremists. But they were in fact more ominous, since the projected German-dominated *Mitteleuropa* was, in Riezler's words, "my new Europe, that is, the European disguise for our will to power" that he felt was prevented only by the extremists' "explicit stipulation of German hegemony" (pp. 108–09).

Riezler may have suggested Lenin's return to Russia and represented the chancellor in making its arrangements. As Foreign Office representative in Stockholm during 1917, he expended extensive funds to facilitate a Bolshevik takeover and negotiated the cease-fire with the Bolsheviks in December 1917. From Moscow between April and August 1918 he unsuccessfully urged Berlin to encourage a bourgeois counterrevolution when he felt the Bolsheviks were losing power. He played an active role as Berlin's representative in the bloody extermination of the communist uprising in Bavaria during 1919. A republican by default after the imperial system's demonstrated bankruptcy, Riezler worked to improve Weimar but—like many fellow cultured, bourgeois intellectuals—never became a convinced democrat. Forced by the Nazis to resign his post at

the University of Frankfurt, Riezler became a member of the "university in exile" at the New School of Social Research.

Riezler's writing on international relations reflected contemporary views and suggests the influence of events on historical constructs. His pre-World War I theories sought to reconcile German idealistic rationalism with widely held notions of a Social Darwinist struggle among national organisms culminating inevitably in conflict. But his experience with the imponderables of decision making, the collapse of the old world and then the new under Hitler, caused him such serious doubts about the comprehensibility of politics that he assumed a proto-existentialist position after World War II: "man—present man—gives meaning or fails to give meaning" (p. 224) to human events that lack intrinsic order. His career likewise reveals the dilemma of the intellectual in politics. Mental acumen gave him access to power but also generally restricted his influence to advisory functions. When involved more deeply in events, he became a less reliable adviser and observer. Apparently one could not be both intellectual and politician. The intellectual's function is to comprehend, whereas—in Riezler's words—the statesman "is called upon not to interpret history but to make it" (p. 223).

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LOUIS DUPEUX. *National Bolchevisme: Stratégie communiste et dynamique conservatrice*. Volume 1, *Dans l'Allemagne de Weimar, 1919–1933*; volume 2, *Documents et tables*. Paris: Librairie Honoré Champion. 1979. Pp. vii, 588; 591–743.

Like many of the anomalous political movements of Europe in the interwar period, National Bolshevism has a rather extensive historical literature on the Continent. To most scholars it has been regarded as a peculiar effort by some extreme right-wing activists to reconcile their political objectives with the tactical methods of the Bolsheviks during the years of the Weimar Republic. Louis Dupeux has undertaken to re-examine virtually all of the relevant source material available in the German archives and to offer a definitive analysis and collection of documents on the subject.

Dupeux challenges the widely held view that National Bolshevism was heavily indebted to Arthur Moeller van der Bruck, a founder of the conservative *Juni-Klub* and author of *Das Dritte Reich* (1922). A far more important forerunner, in Dupeux's view, was Professor Paul Eltzbacher, who made the first articulate statements for the National Bolshevik position in April 1919. Dupeux refutes

the assertion of Fritz Stern (*The Politics of Cultural Despair* [1961]) that Moeller was "the leading apostle of national bolshevism." He emphasizes the ideological contributions of Han von Hentig, Ernest Niekisch (editor of *Widerstand*), and the Hegelian economist Max Lenz.

A significant portion of this long manuscript is devoted to emphasizing the distinctions between the genuine "National Bolshevism," as Dupeux defines it, and other fringe political groups that are often confused with it. The "national communism" movement based in Hamburg, which is frequently associated with National Bolshevism, is shown to be a distinct entity with some superficial similarities but an essentially different ideological source. Even Lenin is shown to have been confused by the apparent similarity between the two types of political movements who had appropriated similar labels.

Contrary to the usual practice of dividing National Bolshevism into three phases, with departure points in 1919, 1923, and 1930, Dupeux identifies two major periods—an early "spontaneous" phase generated by the defeat of 1918 and a later, more intellectual manifestation in the early 1930s, with a rather tenuous connection between the two eras.

The care that Dupeux has taken to define National Bolshevism and to identify its major assumptions is commendable. An ideological movement that drew upon a conservative German affinity for Russia and the "East," together with a preference for specific Bolshevik authoritarian procedures, blended with anti-Marxist, anticapitalist, anti-Western biases, was a strange hybrid indeed. The National Bolsheviks spoke of a "proletariat," but it was quite a different species from that of the left. Their fundamental commitment was to a radical German nationalism, yet they hated the Nazis and fell victim to them.

The publisher has placed Dupeux's text in one volume, with the tables and some sixty documents relating to the movement in a smaller companion volume. The document collection, which includes comments from a variety of contemporary sources, is likely to prove useful to scholars with more general interests.

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TELFORD TAYLOR. *Munich: The Price of Peace*. Reprint. New York: Vintage Books. 1980. Pp. xiv, 1,084. \$8.95.

Telford Taylor has written a lengthy, detailed account of the history of the Munich crisis in over one thousand pages of text. Taylor has aimed at a fresh assessment of the men and the methods that were

used to "resolve" the crisis. He seeks to learn if there is any lesson to be learned from the Munich crisis.

Taylor's book opens with an analysis of the Munich conference and provides a first-rate examination of the sources for the conference. Then he asks: "How and why did this all come to pass?" (p. 67). Turning back to 1918, he analyzes the Treaty of Versailles, Eastern Europe, and Great Britain during the 1930s. Next Taylor traces German military and diplomatic power from the Rhineland coup to the plan for the attack on Czechoslovakia and then the diplomatic crises, 1936–38, as seen from Prague, Moscow, Paris, and London. Starting with Chamberlain's flight to see Hitler on September 15, Taylor tells the story of the September crisis in masterly fashion. In much less detail, he follows the events from September 30, 1938, to the outbreak of World War II.

To Taylor, Neville Chamberlain is the architect of Munich who persisted until he pushed Hitler into agreeing to the Munich conference. Chamberlain, more than anyone else, dominates Taylor's book.

In his conclusions, Taylor argues that Britain and France ought to have gone to war in 1938 when they would have had more allies than in 1939. He assumes that the Soviet Union and Poland would have joined in a war against Hitler if Britain and France had led the way. Taylor also insists that Britain and France ought to have gone to war in 1935 when Italy invaded Ethiopia. Taylor blames the British for restraining the French from invading the Rhineland in 1936 when they "could hold Hitler in check without seriously risking a major war" (p. 1,000). In these instances, and in others, Taylor relies on interpretations that are no longer tenable.

Because so much of his narrative deals with Chamberlain and British policy, Taylor has failed to make full use of the materials in the *Documents diplomatiques français*, which offer an entirely new view of French policy toward Czechoslovakia.

As for the lessons to be learned from studying the Munich crisis, Taylor concludes that Munich "is a potent and historically valid symbol of the dangers of not facing up to unpleasant realities" (p. 1,004). He just might be right.

Although Taylor has certainly written the longest account of the Munich crisis, no student of this international crisis can ignore this book.

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ROBERT FINLAY. *Politics in Renaissance Venice*. New Brunswick: Rutgers University Press. 1980. Pp. xvii, 308. \$23.00.

This is an interesting and significant first book. Examining the complexities of politics in Renaissance Venice, Robert Finlay has brought a synthesizing vision and a wealth of detail to a topic that has long deserved closer study. From a virtually required starting point—the myth of Venice—Finlay sails bravely out onto the rough seas of the reality of Venetian governance. The “constitution” is dispensed with quickly, for Finlay argues cogently that Venetian politics were too fluid to be delimited by formal governmental structure. Rather he uses that structure as a setting for examining the interplay of family, age, and personality. In the process he draws several suggestive conclusions; for example, that the Doge was more powerful than constitutional limitations and previous scholarship have implied; that age was a primary factor in selection for and moderation in office; and that *Broglia*—the illegal electioneering for office and votes—“was the oil that made the complex machinery of state function so smoothly for so long that it seemed that Venice was free from ambition and faction” (p. 221).

Many of his points are not new; the discussion of *Broglia* is reminiscent of a paper delivered at a Warwick *convegno* in the early seventies. But Finlay marshals an extensive scholarship that should move debate significantly ahead. And in the end he comes to the provocative conclusion that the myth of Venice, like all myths, does have an important historical meaning: it “extolled as intrinsic to the constitution that which was an appearance produced by the politics that gave life to the constitutional order” (p. 287). It remains to be seen whether proponents of the myth will accept an interpretation that comes dangerously close to throwing their baby out with the bath water.

A first book that attempts so much cannot be expected to be without fault. Particularly suspect are the discussions of the status of *cittadini* and the dangerously rapid treatment of lower-class attitudes toward the closed government. A potentially more telling problem stems, paradoxically, from the book's real strength. Finlay has drawn his primary material from five noble Venetian diarists: Marin Sanuto, Girolamo Priuli, Pietro Dolfin, Marcantonio Michiel, and Domenico Malipiero. Their fascination with and, in the case of Sanuto, virtually obsessive recording of how they perceived government working provide a vivid picture of political life. But Finlay's approach also allows him largely to avoid a mass of archival material that is only beginning to be investigated. Ultimately, the verdict of archival research may significantly undermine the subjective account presented by these diarists. Aware of this problem, Finlay sums it up best himself: “A study of politics based principally on the diaries cannot pretend to flawlessness. . . . One finally must take refuge in Priuli's justification that it

is better to proceed despite flaws than not to proceed at all” (p. 13). This work demonstrates Priuli's point.

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MELISSA MERIAM BULLARD, *Filippo Strozzi and the Medici: Favor and Finance in Sixteenth-Century Florence and Rome*. (Cambridge Studies in Early Modern History.) New York: Cambridge University Press. 1980. Pp. ix, 197. \$24.50.

In 1508 Filippo Strozzi, born to an illustrious Florentine family, married a Medici heiress, thereby forging a bond that also tied him eventually to two Medici popes and to the coming lords of Florence. Soon his new relations heaped favor on him at Florence and Rome, where he founded banks, got the lion's share of the banking business, and was catapulted into one of the era's great financial empires. His fortunes peaked under Popes Leo X (d. 1521) and Clement VII (d. 1534), until the latter's death sent the courtier-banker scurrying to rescue what remained of his empire.

Although the story of Filippo's financial shenanigans and spectacular rise is complex and devious, Melissa Meriam Bullard tells it with enviable skill in 178 pages. The freshness and importance of her version are based upon arduous archival research in Rome and Florence.

Depositor general for popes as well as for the government of Florence, Filippo concealed his lucrative doings with the help of talented agents and double sets of account books. He also got the connivance of government insiders and key officials in Florence, appointees and followers of the Medici. By this means, he and his Medici patrons converted Florence's tax coffers into a tool of papal policy and nepotism. Hundreds of thousands of gold florins were thus rerouted. Contemporaries were aware of this but could not prevent it, owing to the Medicean control of Florentine public office, including the keeping of the city's fiscal records. Accounts were certainly doctored, yet the record somehow speaks for itself, catching even Filippo's tricky dealings in wet grain supplies and debased coinage.

Indeed, sources are so multitudinous that Bullard was forced to scratch many surfaces, making her book the more suggestive. Her soundings reveal that we do not yet sufficiently understand major aspects of papal finance (for example, the detailed powers of the depositor general). We know too little about the overall cost of the Italian wars and less still about their hobbling effects on the Italian economy. The princely profits of papal bankers, viewed as a function of percentages of interest taken, are still *terra incognita* because interest was disguised in the

official accounts. Again, the history of the Florentine public debt (the *monte*) in the decades around 1500 has yet to be written. When it is, we shall know more about Bullard's dramatic claim that a gulf was opening within the Florentine aristocracy between the many who were merely wealthy and the few whose wealth became fabulous. Properly handled, these questions should occupy teams of historians for years.

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JAN LINDHARDT. *Rhetor, Poeta, Historicus: Studien über rhetorische Erkenntnis und Lebensanschauung im italienischen Renaissancehumanismus*. (Acta Theologica Danica, number 13.) Leiden: E. J. Brill. 1979. Pp. 197. f 60.

The works of the Florentine chancellor Coluccio Salutati have served scholars as a favorite touchstone for testing Renaissance humanism's value and modernity. Among scholarly assessments Jan Lindhardt's work will rank as a fine contribution. Like many other students of Salutati's works, Lindhardt focuses on apparent contradictions in the views Salutati expressed at various stages in his life. Some scholars have perceived these anomalies as outward manifestations of a struggle in Salutati's mind between medieval and humanist ideas. Thus Alfred von Martin spoke of Salutati as a *zerrissener Mensch*. Lindhardt agrees neither with von Martin nor with those who wish to resolve the apparent contradictions by dividing Salutati's works into rhetorical and substantive writings. The most unflattering version of this approach accepts the term rhetoric in its present derogatory connotation and labels Salutati's so-called rhetorical writings ornamental at best, deceptions-to-please at worst. In a more flattering version, rhetoric is a respectable but merely technical enterprise that endows substance with beautiful form.

Lindhardt sides with more recent scholarship that sees rhetoric as a discipline by far exceeding the scope of mere form. He discerns no gulf between the worlds of *eloquentia* and *veritas* or *sinceritas*. Rhetoric—the sister of poetics in Salutati's words—becomes an equal to philosophy and theology. The works of Salutati and his fellow humanists represent serious interpretations of the human life. In the context of that life thought can accommodate what logically seem contradictions. Thus *De seculo et religione*, advocating monasticism, fits with Salutati's call for the *vita activa*. Through a monastic life Christians love God and through the active life they love other humans. Salutati is also seen as a mediator in the great controversy over the respective

status of volition and wisdom. The quest for knowledge can be combined with an emphasis on human will if knowledge becomes knowledge for action's sake. Lindhardt sees rhetoric as transcending the world of pure logic and leading to a truth on human terms, which not only enables modern scholars to resolve the apparent inconsistencies in Salutati's views but also equipped Renaissance humanists to cope with an ever more complex intellectual world. Lindhardt's book enriches Salutati scholarship by its careful analysis and its cautious synthesis. While commendably free of the verbal fog sometimes encountered in this field, its exposition is at times a bit lengthy, and its German text should have had better proofreading.

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DOMENICO SELLA. *Crisis and Continuity: The Economy of Spanish Lombardy in the Seventeenth Century*. Cambridge: Harvard University Press. 1979. Pp. ix, 255. \$18.50.

In this elegant and well-written study, Domenico Sella sets out to modify and refocus the perceptions of Spanish Lombardy embedded in the available literature. In his view, that literature contains three major flaws: an excessive emphasis on the urban economy, a failure to identify the causes of the seventeenth-century crisis, and a tendency to portray the crisis as an "irreparable break in the economic and social texture of Lombard history."

An introductory chapter detailing the period of prosperity subsequent to the treaty of Cateau-Cambrésis of 1559 provides the author with an opportunity to lay out the economic geography of the territories of Milan and to assert the significance of the agricultural base in sustaining that prosperity. The transition to overpopulation, food shortages, and the growing dependence of northern Italy on Baltic grain at the beginning of the seventeenth century is an easy, logical one that serves to introduce the period of crisis. Diffusion of technological skill and the consequent loss of markets supplemented food pressure in troubling Milan, but these negative forces were only a prelude to the seventeenth-century disruptions marked by plague, depopulation, war, and agricultural and industrial dislocation. In a carefully reasoned chapter, Sella endeavors to allocate blame for economic decline among taxation, war, and foreign competition. In doing so, he notes that the aggregate impact was far more severe in the cities than in the countryside and that the countryside exhibited a healthy resilience during much of the dark seventeenth century. Even rural industry appears to have resisted decline, to have found sources of capital, and even to have benefited from

some expansion and innovation. Not surprisingly to those familiar with the literature, the more liberal attitude found in the countryside stands in contrast to the traditional and ever more repressive posture of the cities. Interestingly, however, Sella finds that the shift in Milan's government to a patriciate with all the attributes of a landed aristocracy may have forwarded the vitality and development of rural industry, since the patriciate acted to defend the interests of the countryside against the restrictionist policies of the urban guilds and tradesmen. The book concludes with a "postscript on feudalism" that, although informative and well argued, seems truly to have been appended as something of an afterthought.

Of the three tasks that Sella set himself, he is perhaps least successful in identifying and assigning relative weights to the causes of the crisis. Within the limits imposed by the available evidence, the arguments are cogent and well presented. Taxation, for example, did have a negative effect, but through a more complex route than is generally conceded, since more Spanish revenue may have been spent in Milan than was collected there in taxes. So far, so good; yet one is left vaguely dissatisfied as to the relative impacts of taxation, war, plague, and restrictionism. There is simply not sufficient surviving evidence to provide substantial answers to these questions. On the other hand, the book is most illuminating and rewarding in re-emphasizing the importance of agriculture and rural industry and in establishing continuities between the sixteenth and the eighteenth centuries. In sum, this is a compelling and important work with implications that reach well beyond Spanish Lombardy.

HARRY A. MISKIMIN
Yale University

LUIGI FIORANI. *Il Concilio Romano del 1725*. (Biblioteca di Storia Sociale, number 7.) Rome: Edizioni di Storia e Letteratura, for the Istituto per le Ricerche di Storia Sociale e di Storia Religiosa. 1977. Pp. 322. L. 20,000.

Luigi Fiorani's exemplary monograph is mostly a reconstruction of the facts in their historical setting of a hitherto little-known provincial synod. The Council of Trent had issued a call for such synods, but due to various circumstances—the turbulence of the times, disinterest, fear of "democraticismo ecclesiastico" (p. 25)—the practice fell into progressive disuse. Among other things, it seems, bishops did not care to advertise their own inadequacies.

Vincenzo Maria Orsini (1724–30) hoped to change all that, if he could. Elected Benedict XIII in 1724, this devout and learned Dominican (he is said to have somehow read Baronius' *Annals* more

than twenty times!) had been the zealous bishop of various dioceses and had convoked numerous synods as bishop of Benevento. He accordingly called a synod shortly after his election, timed with the jubilee year of 1725. Basically a pastoral council of the south central Italian dioceses, it produced 104 new and worthy canons on such matters as the episcopacy, preaching, the formation and residency of clerics, and clarification of the elusive intricacies of ecclesiastical discipline. But its main originality, as Fiorani says, was in Rome's giving an "example of exceptional value" (p. 7) by suggesting the possibility and even urgency of the restoration of the practice of synods and of communal responsibility.

The synod is reconstructed in its historical setting. The most taxing international issue was the continuing controversy in France over *Unigenitus*, the bull of Benedict's predecessor, Clement XI, condemning Jansenism (or more exactly, Quesnel). It was rumored that Benedict had opposed the issuance of *Unigenitus*—he had held to the doctrine of efficacious grace—but it was soon clear that he would support the bull in any negotiations with the party of the *appelants* in France. The other related issues were a jurisdictional conflict with Naples, never too keen on the council, and polemics from the political antiromanist, Pietro Giannone, and the Lutheran, Johann Georg Walch, for whom the council was too Tridentine. For Fiorani, the council could not be successful because ultimately, "the serene and satisfied imperturbability of the conciliar fathers" (p. 221), seated in the glorious isolation of the Sistine Chapel, was too far removed from the stark realities of the ignorance and deprivation of the people and the clergy in the mountains and the countryside of Italy.

Enormously scholarly and reinforced by almost a hundred pages of appendixes, this is apparently the product of more than a decade's research. Interestingly, it seems to have emerged in the circumstances of John XXIII's celebration of a Roman synod in 1960. Implicitly, I believe, it is more than scholarly—and most welcome.

DONALD NUGENT
University of Kentucky

RICHARD DRAKE. *Byzantium for Rome: The Politics of Nostalgia in Umbertian Italy, 1878–1900*. Chapel Hill: University of North Carolina Press. 1980. Pp. xxvii, 308. \$20.00.

This first book by a young scholar discusses a complex period in the history of liberal Italy. One wishes it were better, for it rests on sound research. Unfortunately, the subtitle promises more than the book delivers, and many of Richard Drake's generalizations about Italian history raise questions

about his interpretation of it. In fact, Drake deals in depth only with the *Cronaca bizantina*, a literary journal founded in the early 1880s by Angelo Sommaruga. A better subtitle might have been "Sommaruga, His Friends and Enemies, 1881-85," for when writing about this episode in Italian publishing and literary activity Drake does indeed make a real scholarly contribution. Otherwise, he is on shaky historical ground in trying to tie together in one neat package the *Cronaca bizantina*, Gabriele d'Annunzio's later political activity (presaged already by his articles on the naval appropriations bill in parliament, which Drake fails to mention), and Enrico Corradini's burgeoning nationalism. Many other factors and events, besides the short-lived *Cronaca bizantina*, molded the later outlook of both men. Thus, the *Cronaca bizantina* remains but one expression of discontent, among many, with the prosaic and materialistic society of modernizing Italy. Sommaruga's publishing entrepreneurship reflected this new spirit, but Sommaruga's importance in Italian life ended in the mid-1880s under a cloud.

Drake's writing style is in turn gushy, colloquial, and pretentious. Gossipy details on the personal appearance, duels, and love affairs of intellectuals and irrelevant bits of information, such as his account of the Paris meeting in 1912 between d'Annunzio and Sommaruga, add little to the reader's understanding of the "politics of nostalgia." Words are used curiously. What exactly are "invincible cynicism" (p. 31) and "invincibly . . . held opinions" (p. xxii)? Translations from the Italian occasionally misinterpret the original meaning: *superbamente solo* is rendered as "superbly alone" (p. 93) rather than correctly as "proudly alone." There is a tendency, perhaps unconsciously borrowed from d'Annunzio, whose work Drake uses extensively, to hyperbolize. In youth, Giuseppe Mazzini is likened to "prince Solomon"; in age, to the "prophet Hosea" (p. 8). Giosuè Carducci becomes the "Italian Aeschylus" (p. 9). Victor Emmanuel II is credited with "Olympian achievements" (p. xx).

Despite these criticisms, the book does add a chapter on a previously neglected topic to the growing literature in English on modern Italy. Better general and more extensive studies of the pre-1914 dissatisfaction felt by the Italian intelligentsia with their country's political system may, however, be found in Armand Patrucco's unpublished Columbia University dissertation on antiparliamentary thought in post-1861 Italy, which illustrates the broader range of the "politics of nostalgia," and in John A. Thayer's *Italy and the Great War: Politics and Culture, 1870-1915* (1964), in which intellectual discontent and politics are analyzed in greater depth.

EMILIANA P. NOETHER
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NARCISO NADA. *Dallo Stato assoluto allo Stato costituzionale: Storia del Regno di Carlo Alberto dal 1831 al 1848*. (Pubblicazioni del Comitato di Torino dell'Istituto per la Storia del Risorgimento Italiano, new series, number 8.) Turin: Istituto per la Storia del Risorgimento Italiano, Comitato di Torino. 1980. Pp. 182. L. 10,000.

Even before his accession Carlo Alberto was controversial, for his role in the abortive revolution of 1821 was regarded as traitorous by liberals and heroic by conservatives. His eighteen-year reign and abdication in 1849 added fuel to an argument that each generation of historians perceives anew. Although he provides a meticulous analysis of the debate, Narciso Nada avoids the pitfalls of a mediating role by concentrating not on the personality of Carlo Alberto but on the evolution of Piedmontese institutions and the character of the king's ministers. A portrait emerges of an administrator whose measures did not arise from avowed liberalism but whose personality assumed a liberal aura because of his choice of moderate ministers, especially after 1841. Beginning with foreign policy, Nada argues that by endorsing legitimacy and condemning secret societies (especially Mazzinian ones) early in his reign, Carlo Alberto protected his independence from Austrian interference. Thus he could pursue a cultural *italianità* and moderate institutional reforms, policies that might contribute to a traditional goal—the expansion of the Savoyard state.

The passage from an absolutist to a constitutional state, affirmed in the *Statuto* of 1848 that Carlo Alberto bequeathed to his successor (and to the subsequent Italian state), is attributable largely to the king's selection of men, to the collegiality of the Council of State, and to growing political pressures exemplified by 1847 in the liberal demands of Genoese delegations. Nada, interested in the modernizing nature of reforms, shows that what Camillo Cavour and Victor Emmanuel II inherited after 1849 was not only a centralized state but also one in which communal and provincial reforms affected important elements of the *borghesia*, especially the entrepreneurial class. The codification of civil, penal, and commercial laws reflected the innovative and Napoleonic orientation of men like Giuseppe Barbaroux, who headed the commission for this task. Had not the revolution intervened, judicial reforms on the eve of 1848 would have led to elections on a communal level open to small proprietors and artisans. Nada perceives the infrastructure of a modern economic state as created by men with *laissez-faire* tendencies who sought to diminish protection and to promote industries, new banking facilities, and various railway projects. University chairs of public and commercial law were created (although steps toward secularization of education

were limited to avoid confrontation with the papacy).

The focus on institutions and ministers reveals a society in the process of modernization, a phenomenon more complex and significant than a king's personality. Nada might have stressed one lesson that Carlo Alberto, perhaps unfortunately, taught his successors: that centralization and uniformity were the appropriate response to rebellious regions. Such was Piedmont's reaction to the problem of Genoa. Still, this is an important contribution, and one may anticipate a valuable sequel on Carlo Alberto in 1848-49.

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CAROLINE WHITE. *Patrons and Partisans: A Study of Politics in Two Southern Italian Communi*. (Cambridge Studies in Social Anthropology, number 31.) New York: Cambridge University Press. 1980. Pp. xii, 196. \$19.95.

The unabashedly relevant stance and partisan tone of Caroline White's study in social anthropology in no way detract from the value of her book as a work of history. The story itself has all the power of an Italian folktale. Once upon a time in Fucino, the land of Ignazio Silone's *Fontamara*, the fishermen of Luco worked in harmony, plying their boats along the lake and drawing their nets ever closer upon the catch. Their sturdy wives quickly gutted and packed the fish so that swift runners might bring them as far as Rome within just eighteen hours. Visitors commented on the unusual friendliness and industriousness of the townspeople, surely a result of "this relatively equal distribution of productive resources" (p. 87).

Then in the 1860s the wicked Prince Alessandro Torlonia came along and drained the entire lake. He leased the miraculously fertile soil in large allotments to outsiders—who in turn subleased at exorbitant rates to other outsiders flocking in from even poorer regions further south—and vanished in wealthy splendor to Rome, where his descendants became ardent fascists. Meanwhile the *luchesi*, although temporarily seduced by a six-year windfall of low but steady wages, pushed Torlonia's markers (showing what would be his when the lake was drained) into the water. They desisted only when new markers were adorned with cast iron statues of the Madonna. For nearly a century the *luchesi* fought the Torlonia regime: in the 1870s over water rights; in 1913, when fieldhands struck for higher wages; two years later, when women attacked army grain requisitioners; during the "Red Years" after the war; under fascism, when two of Luco's own

were beaten to death; and in the late 1940s, when the Communist party (PCI) opened consumer cooperatives, ran adult education classes on Marx, Lenin, and Gramsci, and organized communal snow-clearing operations. Luco had become the center of the "Red Belt of the Abruzzo," and in 1951 the struggle for Fucino ended in "victory." The Demochristian (DC) government expropriated Torlonia's vast holdings and assigned them to his former subtenants, countering peasant radicalism by creating a class of smallholders while simultaneously alienating daily wage farm laborers who had expected more from the PCI than acquiescence in a plan that undermined collective agriculture. To this day the PCI and the DC in Luco engage in active, stable, open competition. People vote for what they think is right.

Just a few kilometers away in the town of Trassacco is the all too familiar (Sidney Tarrow, Edward Banfield, Sydel Silverman, Alex Weingrod, Eric Wolf) story of clientelism, in which individuals seek power within ideologically bankrupt parties by handing out as favors things that should have belonged to their clients in the first place. The clients, now as in the past, are poor folk for whom survival is a daily struggle and who depend heavily on kin networks and on their betters. The incompetent doctor, the unholy priest, and the parasitic lawyer take not only votes in exchange for their precious letters of recommendation but also the people's "right to engage in class struggle" (p. 5). White concludes on an optimistic note, with the hope that her study will hasten in some small way the demise of clientelism by awakening the DC, which claims to oppose such political practices, to its evils. That is doubtful—the DC faithful seem not to believe in folktales—but at least we now have the most thorough, and most impassioned, account of postwar Italian local politics I have ever read.

RUDOLPH M. BELL
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ALAN J. REINERMAN. *Austria and the Papacy in the Age of Metternich*. Volume 1, *Between Conflict and Cooperation, 1809-1830*. Washington: Catholic University of America Press; distributed by ISBS, Forest Grove, Ore. 1979. Pp. x, 254. \$24.95.

This illuminating study is the result of extensive research in many documentary collections. Some of Alan J. Reiner's findings have appeared in published articles, but this clearly written volume (the first of two) puts it all together in a well-analyzed perspective. The author's conclusions—perhaps articulated a bit too often—clarify the relationship of Prince Clemens von Metternich and papal statesmen in the post-Napoleonic restoration

period. Once again a thorough archival investigation reveals the Austrian chancellor as an intelligent and perceptive moderate exasperated with reactionaries doing their best to bring about a revolution. The papal state had its full share of such types, and Reinerman shows how both Metternich and the adroit Cardinal Ercole Consalvi—the ablest of papal moderates—tried to preserve the union of throne and altar, which they considered vital for European stability. Their task was made immensely difficult by Viennese Josephists, suspicious of papal pretensions, and by Roman *zelanti*, suspicious of Austria's Italian ambitions. Metternich worked hard to dismantle the heritage of Joseph II to which the Emperor Francis seemed to cling; Consalvi tried to assure his colleagues that Austria was more a friend than a foe. Obviously, the actions of the Austrian military during the Neapolitan revolution tested the cardinal's credibility, and the Austrian chancellor just could not get his procrastinating monarch to disavow once and for all the Josephist state-church system. Furthermore, Metternich's attempts to urge reforms on the papal administration not only aroused the *zelanti* but his proposed methods for controlling revolutionary sects effectively throughout the peninsula also provoked doubts among the Roman moderates. Pius VII's condemnation of the Carbonari was for Metternich only a partial success in a general pacification policy that demanded order at home as well as abroad.

Enlightened cooperation between Rome and Vienna ultimately was to sag in Consalvi's last year, only to be revived gradually during the pontificate of Leo XII. Such international church-state crises as ultramontanism, which attacked the union of throne and altar, forced Rome to make choices and to renew its faith in Habsburg Austria. Yet, Reinerman feels, the decision to forsake the "potentially fruitful line of development" (p. 142) that ultramontanism offered was a natural one when considered in the context of the restoration. Once again the interests of Rome and Vienna were parallel. Ironically, by abandoning Metternich's suggested reform, the papal government also jeopardized the very independence it so ardently had sought to preserve. Only Austria could now save the papal state from certain revolution, and revolution came soon enough, during the short pontificate of the pro-Austrian Pius VIII.

Basically, Reinerman's work is less a re-evaluation than a fresh start, and the great bulk of his innumerable references are original correspondence. (The occasional citations from Metternich's published papers probably should have come from the original German-French rather than the French edition.) Unfortunately, a number of bothersome petty errors—typographic and other—were not eliminated before the final printing (the name of

Carl Cajetan Graf von Gaisruck, the archbishop of Milan, for example, is constantly misspelled). Also, a map detailing the various Italian principalities of the time would have been very helpful. Let us hope that somewhat more care will be taken in the forthcoming concluding volume of this useful work.

ARTHUR G. HAAS
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HARALD BACHMANN. *Joseph Maria Baernreither (1845–1925): Der Werdegang eines altösterreichischen Ministers und Sozialpolitikers*. Neustadt a. d. Aisch: Kommissionsverlag Ph. C. W. Schmidt. 1977. Pp. 178. DM 38.

This is a political biography of one of the ablest and most active parliamentarians of the last four decades of the Habsburg monarchy. It covers all the main aspects of Baernreither's career: his research into social developments and social problems ranging from peasant inheritance in Germany and the trade union movement in England to juvenile delinquency in America; his activities in the Reichsrat, both as social and legal reformer and as leader of the Liberal great landowners' parliamentary group; the part he played in the endless negotiations for a settlement of the national conflict in his homeland of Bohemia; and his efforts as a publicist during the 1900s to turn Austria-Hungary onto a course of friendship with the South Slavs both inside and outside its borders.

Harald Bachmann brings together a large amount of detailed information, some of it from Baernreither's personal papers, especially his voluminous political diary. Bachmann makes particularly good use of this material in his chapter on the Bohemian question, where he also draws on his own specialized knowledge of Bohemian German personalities and politics of the period. Here he both elucidates the administrative and legal complexities of the national conflict and also draws attention to the realism, and even sympathy, with which Baernreither accepted the rise of the Czech nation, even while he tried to safeguard the Bohemian Germans against it.

As a whole, however, the book is unsatisfying to the reader. Bachmann's biographical method throughout is to report Baernreither's deeds, projects, and opinions, express approval and admiration of their statesmanlike qualities, and deplore (as all too often he must) the fact that the paralysis of the Reichsrat or the incompetence of the government prevented this, that, or the other project from being realized. In all of this, Bachmann leaves a great deal about Baernreither unexplained. It is by no means clear what Baernreither's plans for himself in politics were or how he hoped to achieve them. We see

him at one point refusing an offer of a chair at the University of Vienna for the sake of his political career and then refusing an offer of a portfolio from Ernst von Koerber. We see him scoffing at Koerber's suspicions that he wanted to replace him as prime minister and years later regretting that he never had been prime minister. Nowhere does Bachmann explain the meaning of these contradictory ambitions, refusals, disavowals, and regrets.

It seems to this reviewer that Baernreither did in fact badly want office, including the highest office, that statements of his to the contrary are signs of mere temporary despondency or of unwillingness to avow hopes and plans that had come to nothing, and that his activities as a researcher and a publicist were a means to this end, or perhaps at times a substitute for it. If this is true, it raises the further questions of why, in the more than twenty-five years during which he was generally considered "ministrel," Baernreither only held office for a total of about a year and whether he imagined that as prime minister he could have solved the Bohemian question, persuaded the Hungarians not to stand in the way of an accommodation with the South Slavs, and done whatever else was necessary to save the monarchy. If Bachmann had discussed problems of this kind, his answers would certainly have given us valuable insight into Baernreither's character as a statesman, the significance of his career, and the politics of Cisleithanian Austria in which he played such an important role. Without this, Bachmann's book does only part of what a political biography is supposed to do.

GAVIN LEWIS
John Jay College
City University of New York

HEINRICH LUTZ. *Österreich-Ungarn und die Gründung des Deutschen Reiches: Europäische Entscheidungen, 1867-1871*. Frankfurt am Main: Propyläen Verlag. 1979. Pp. 608.

This detailed and authoritative work evaluates the policies of Count Friedrich Beust, the Habsburg monarchy's Reich chancellor after Austria's defeat in 1866. Heinrich Lutz, with great care, clearly defines these policies, shows how they were shaped and changed by internal developments within the Austrian empire, and considers their effect on the rest of Europe.

Lutz shows that Beust had tendencies toward accepting certain federalistic corrections to deal with domestic multinational questions. Nevertheless, he strongly supported the idea of dualism, that is, a German-Hungarian cooperation against the Slavs. This dualism, in which the German element controlled the central government, was coupled with a

foreign policy aimed at regaining a significant Austrian position in Mitteleuropa.

Until 1871 Beust was in no way resigned to a secondary role for Austria in Germany. His major objective was to carefully bolster anti-Prussian currents in south Germany, to establish there an Austrian-controlled confederation, and thus to win parity with Prussia in German affairs. The logical ally in achieving this seemed to be France, but the possibilities of obtaining a strong Austrian-French diplomatic combination vanished on the basis of unsureness and a lack of mutual, genuine trust. Napoleon III wanted an alliance that would commit Austria to a war against Prussia. Beust could not accept that without losing the support of German nationalists in southern Germany and at home. His subsequent hopes for increasing his bargaining power with Prussia by establishing an Austrian-French-Italian coalition were dashed by the Roman question and the refusal of France to evacuate its troops protecting the pope.

The position of Austria in Germany also became closely linked to Eastern affairs. Austrian-Russian relations badly deteriorated after 1867, the gap widening because of pan-Slav propaganda within the monarchy and because both powers were diametrically opposed on how to deal with the Turkish domination of Balkan peoples. Subsequently, both Prussia and Russia went on the offensive in their respective spheres and ended standing back-to-back against a common enemy—the Dual Monarchy.

A definite change in Austrian-Prussian relations began in the fall of 1869, for Bismarck had now decided that his major enemy was France, not Austria. As Beust had feared, south Germany responded to a French provocation of Prussia by joining the North German Confederation. With his plans for south Germany shattered, Beust tacked about, and in 1870 he began what became an irreversible rapprochement with Berlin. Even now he failed to achieve what he wanted: an Austrian-German alliance aimed against Russia. His successor, Julius Andrassy, would sign the Three Emperors' League in 1873.

In the end, Beust was thwarted in each direction he turned. This is hardly surprising given the elements with which he had to work: a Bismarck who understood the intricacies of power, an unastute French emperor, and an Austrian monarchy that had still not decided on a federalistic, or a dualistic, or a pan-Slav solution to its internal and external problems.

Based as it is on exhaustive research and deep knowledge, and notwithstanding its rather verbose style, Lutz's monograph is a fine addition to existing literature on the subject.

GERARD E. SILBERSTEIN
University of Kentucky

JOSEF AUSSENMAIR. *Kirche und Sozialdemokratie: Der Bund der religiösen Sozialisten, 1926-1934*. Introduction by FLORIAN KUNTNER. Epilogue by JOSEF WEIDENHOLZER. (Schriftenreihe des Ludwig Boltzmann Instituts für Geschichte der Arbeiterbewegung, number 10.) Vienna: Europaverlag. 1979. Pp. 238.

There are those who think that Christianity and socialism, both properly understood, are indeed complementary. But neither the Social Democrats nor the Catholic church in the First Austrian Republic shared that view. The "Religious Socialists," led by "little" Otto Bauer (to distinguish him from the leader of Austromarxism), were attacked from opposite sides by the majority of the church hierarchy and by the bulk of uncompromising Austromarxists, especially the "Free Thinkers."

This book by Josef Aussenmair is the answer to the demand of those who have waited for years for a scholarly work on the history of the Religious Socialists, the term applying only to Christians, primarily Catholics. Let it be said straightaway: it is a very good book indeed. It opens with a discussion of socialist Christians (Christian socialists might be slightly misleading in this context) in Switzerland and Germany and of the theological justification of a reconciliation of Christianity with socialism, Paul Tillich and Karl Barth being, very appropriately, the chief spokesmen of such a theology.

The main body of the book concerns the founding of the Religious Socialists in 1926, the impact of the papal encyclical *Quadragesimo Anno* of 1931 (which had also a potent influence on the corporate state advocates), and the dissolution of the Religious Socialists in 1934. Moreover, Aussenmair also discusses the Religious Socialists' critique of both socialism and Christianity and their effort to bring both onto the same track, emphasizing that which unites rather than that which divides the two. A recent interview in a Viennese daily with Otto Bauer, who has lived in America since before World War II, has aroused the interest of contemporaries in the not very successful endeavor to bring socialism and (Catholic) Christianity together in the twenties and thirties. The endeavor was predictably doomed because of the intransigence of both the clergy and the atheistic wing of the Social Democratic party. Which was more adamant remains a matter of opinion.

Aussenmair's book is the product of a conscientious scholar. He is equally at home in theology and in recent Austrian history. The book comes equipped with an excellent bibliography and a highly informative appendix, which no serious student of the subject would want to miss. If in the Second Republic church and socialism get on distinctly better than they did a generation ago, the groundwork for the present detente was laid by the

courageous and committed men and women who affirmed socialism but would not give up their Christian loyalty, who in fact regarded themselves as socialists, not despite their Christian commitment, but because of it.

ROBERT SCHWARZ
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JON V. KOFAS. *International and Domestic Politics in Greece during the Crimean War*. (East European Monographs, number 61.) Boulder, Colo.: East European Monographs; distributed by Columbia University Press, New York. 1980. Pp. 174. \$12.00.

The author, Jon V. Kofas, has scrupulously probed the archives of the Greek, English, and French Foreign Offices to produce this concise volume describing Greece's domestic and international politics during the period from 1844 to 1857. This study is divided into five chapters surveying the English, French, and Russian roles in Greece, which culminated in the Anglo-French occupation of the country, 1854-57. A conclusion of five pages summarizing the author's views is followed by an appendix, notes, bibliography, and an index.

Although the theme of this work concerns Greek foreign policy during the Crimean War, the author believes that the events of 1854 were predetermined by the parliamentary dictatorship of John Kolettis, whose passionate and dogmatic attachment to the *Megali Idea* led to controversy, recrimination, intrigue, and schism in Greek life. At the other extreme, Alexander Mavrokordatos, with a self-confident temperament, sought a program of internal development of Greece's resources as a preparation for realizing the "Great Idea." The widening breach between these two leaders over the issue of territorial expansion nurtured divisiveness in Greek society leading to the tragic defeat of 1854, which could have been avoided if Mavrokordatos "had remained in power" in 1844.

The period of protracted political maneuvers preceding the outbreak of the Crimean War witnessed the straining of Anglo-Greek relations; the Greeks sympathized with the Russians in opposing Western attempts to preserve Turkey, the hereditary enemy of Hellas. Both the British and French governments, however, reacted immediately to Greek disturbances in Thessaly and Epirus in 1854 by occupying Greece. Totally cut off from outside assistance, the Greek government was forced unwillingly to abandon territorial ambitions.

Throughout the period the lower classes favored expansionist policies. No vision was too unrealistic to excite their enthusiasm. Conversely, the blockade caused economic dislocation for the middle class, which opposed expansionism. The differences be-

tween classes acted as a wedge, driving them apart at a moment when confidence was needed to unite the nation. King Otho, who mobilized sentiments of national pride against his rivals, sought expansion "for his own glory and rejected reform and development in a country which needed it the most." Certainly, even though Otho lacked foresight and control over events, he nevertheless attempted desperately to guide his volatile people and establish some order and security.

Space and format may have forced the author to give only a representative picture of his thesis. The book lacks careful editing; it contains innumerable typographical mistakes and is missing one footnote. Despite these shortcomings, it is a useful study—instructive and reasonably well researched. Although one may disagree with some of Kofas's conclusions, few will deny that he has added, albeit sketchily, to our knowledge of Greek politics during the Crimean War period.

WILLIAM PETER KALDIS
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ELENA GROZDANOVA. *Bŭlgarskata selska obshtina prez XV-XVIII vek* [The Bulgarian Rural Commune from the Fifteenth to the Eighteenth Centuries]. Summaries in Russian and French. Sofia: Izdatelstvo na Bŭlgarskata Akademiia na Naukite. 1979. Pp. 207. 2.20 lv.

Eight years ago, when academician Khristo Khristov published his study of the Bulgarian village commune during the nineteenth-century national awakening, he called attention to the fact that the medieval antecedents of this institution as well as its role during the dark period of Ottoman domination had so far received no attention from historians. The present book by Elena Grozdanova attempts to fill the gap with respect to the Ottoman period. Her work is interesting both in itself and as a reflection of recent tendencies in Bulgarian historiography.

Grozdanova has examined Ottoman judicial and administrative records in the major archives in Sofia and Istanbul as well as smaller, local collections scattered throughout Bulgaria, supplementing these with travelers' accounts, records of Jewish communities, and ethnographic studies. She uses this material most successfully to place the village commune in the network of Ottoman and traditional institutions and to elucidate its functions. She argues that the commune was useful both for the Ottoman authorities, who imposed collective responsibility on the village for taxes and a host of other obligations, and for the villagers themselves, who frequently acted collectively to appeal to the authorities, conclude loans, erect church buildings, and settle internal disputes. Unfortunately, the

reader will probably be disappointed that Grozdanova did not do more with her sources to bring the village to life. Neither notes nor text indicate any familiarity with Western studies in rural sociology, anthropology, or quantitative methodologies, an awareness of which might have made this work more insightful.

Space does not permit a discussion of the intensification of national feeling in Bulgaria that has been so strongly encouraged by the cultural bureaucracy under the direction of Todor Zhivkov's daughter, Lyudmila. One may see its influence, however, on many of Grozdanova's arguments. She writes that village leaders, although they were usually the more prosperous members of the community, should not be seen as exploiters of their neighbors but should be given credit for accepting the responsibility and risk of representing them (pp. 89–102). Traditional peasant narrowmindedness, distrust of the outside world, and resistance to change were major virtues, contributing to the preservation of national identity (p. 155). Christianity, freed of its obligation to justify the oppression of the authorities, merged with the national spirit (pp. 133–34). During the eighteenth and nineteenth centuries, when the commune lost its economic *raison d'être*, its vitality remained strong because the people cleaved to it as an expression of their national feeling (p. 130).

In the end Grozdanova is primarily concerned to see the village commune as the principal vehicle for the preservation of "the heart of the national past" (*narodnata korennata starina*). The phrase is borrowed from Lyuben Karavelov, one of the shapers of Bulgarian nationalism in the nineteenth century to whom contemporary Bulgarian historians are increasingly turning for inspiration.

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ZYGMUNT RUTA. *Szkolnictwo powszechne w okręgu szkolnym krakowskim w latach 1918–1939* [The Elementary School System in the Cracow School District, 1918–39]. Summary in English. (Prace Komisji Nauk Pedagogicznych, number 19.) Wrocław: Osolineum. 1980. Pp. 199. 45 Zł.

Polish education in the interwar period is one of the most popular topics of historical research in Poland. To supplement broad surveys, Zygmunt Ruta has described elementary education in the Cracow school district (encompassing Cracow and Kielce provinces) during the interwar years. He focuses on the organization of elementary education, the push for universal attendance, efforts to reduce the teacher-pupil ratio, and school construction. The author deals more briefly with a variety of topics

ranging from school supplies to special education. Ruta treats each of these topics within six distinct chronological periods. By using such narrow chronological divisions, he masks the changes taking place in Polish elementary education.

Ruta's study suggests a simpler chronology than the one he actually employs: from 1919 to 1929-32 and from 1929-32 to 1939. During the first period serious economic problems, government austerity programs, and a constant decline in the number of school-age children affected educational policy and its implementation. The attempt to create a unified elementary educational structure was compounded for the Cracow school district because Kielce province had been part of Russian Poland and most of Cracow province had been in Austrian Poland. Universal education was a major goal, and Ruta points to the gradual increase in the proportion of school-age children in school in Kielce province and the constantly high percentage attending school in Cracow province. Further, the teacher-pupil ratios declined gradually in both provinces, with Cracow province having one of the lowest ratios in Poland. Lack of state funds and the limited tax powers of local government, as well as the impoverishment of much of the populace, meant that the school construction program made only minimal advances.

The depression, the demographic bulge that reached school age beginning in 1929, and the educational policies of the "Sanation" (*Sanacja*) governments (gradually developed between 1929 and 1932) produced significant changes in elementary education. The percentage of school-age children not in school increased dramatically, the teacher-pupil ratio increased, and the school construction program lagged further and further behind. In addition, the content of education was changed, as inculcating patriotism and loyalty to the regime became the primary purpose of elementary education. Finally, the *Sanacja* governments moved away from the more democratic emphasis on mass elementary education to a policy of promoting elite education at the secondary level.

Meant as a study of the application of state educational policy in one particular region of Poland, the book seldom makes clear what made elementary education unique in the Cracow school district, which included the major intellectual and Catholic center of Cracow, isolated and impoverished villages, and areas of rapid industrialization and urban growth. Nor do the actions and views of local educators called upon to implement the policies emerge clearly. Finally, Ruta's concentration on detail prevents him from achieving his principal stated goal of explaining changes in educational policy.

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KRZYSZTOF DUNIN-WĄSOWICZ. *Ruch oporu w hitlerowskich obozach koncentracyjnych, 1933-1945* [The Resistance Movement in Nazi Concentration Camps, 1933-45]. Summary in English. Warsaw: Państwowe Wydawnictwo Naukowe. 1979. Pp. 446. 130 Zł.

This is a fascinating volume that should be obligatory reading for all those interested in what life was like in the German concentration camps, mostly during World War II. The underlying research is impeccable and meticulous. Krzysztof Dunin-Wąsowicz, a modern Polish historian and himself a former inmate of one of the most infamous camps in Stutthof, has not only examined archival materials in Poland, East and West Germany, France, Holland, and Austria but has also gone through what appears to be a most exhaustive collection of published secondary sources in various languages.

The title of the volume is somewhat misleading. Although its focus is primarily on passive and active resistance within the camps, the book's coverage is much broader and includes a thorough and fascinating discussion of such aspects of prisoners' existence as medical care, religious and cultural life, sabotage, and escapes to freedom.

The author concludes that only between 5 and 10 percent of the prisoners participated in resistance activities against the Germans, the rest concentrating on sheer survival. The most active among the resisters appeared to be the German political prisoners, followed by the Poles and Russians. The Germans were the veterans among the inmates, most of them having been incarcerated in the early 1930s, shortly after the Nazi takeover. Next to the Jews, the Poles represented the largest ethnic group in camps that included members of the military anti-German resistance, captured in occupied Poland. Finally, the Soviet inmates consisted mostly of former German prisoners-of-war who apparently maintained military discipline and a remarkable esprit de corps that enabled them to put up strong resistance. The Jewish prisoners, although most numerous, were usually sent to mass extermination camps where resistance was almost impossible to organize, if only because of the rapid turnover in the inmate population.

To repeat, this is a superbly researched volume that makes an important contribution to our knowledge of World War II and of one of the darkest chapters in recent German history.

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ALFRED ERICH SENN. *Jonas Basanavičius: The Patriarch of the Lithuanian National Renaissance*. (East European Biography Series, number 1.) Newtonville, Mass.: Oriental Research Partners. 1980. Pp. 93. \$9.50.

This short and carefully documented work, although a biography and not a study of nationalism as such, would have benefited by some comparisons between Basanavičius and the founders of nationalistic movements among the other smaller nationalities of Eastern Europe. A comparative framework such as the one laid out, for instance, in Miroslav Hroch's *Die Vorkämpfer der nationalen Bewegung bei der kleinen Völkern Europas* (1967), no doubt would make this remarkable man somewhat less unique than Alfred Erich Senn wishes him to appear. Yet if there is an absence of the kinds of sources that make a searching psychosocial biography possible at this time—as is the case with Basanavičius—a comparative perspective can serve to give a wider meaning to the available details and even perhaps provide an opportunity for suggesting explanations of why nationalistic activists such as Basanavičius did what a chronicle of their lives shows them to have done. All of them, of course, came to their “mission” in different ways; but the questions of why they came to it at all, and why some people as well educated and as knowledgeable in matters of culture did not pursue that mission with as great a fervor, remain unanswered.

A medical doctor by training (he had already left a mark on the development of public health in Bulgaria, where his earlier travels had led him), Basanavičius was present at every important event in the rise of the modern Lithuanian state. He attended the founding of the pioneering newspaper *Ausra* (*Dawn*), which he edited (1883); the founding of the Lithuanian Learned Society (1907), which helped to consolidate the interests of the Lithuanian intelligentsia; and the proclamation of Lithuanian independence in 1918. Nationalist activists in Lithuania had a rougher time of it than their counterparts among the other Baltic peoples, the Latvians and Estonians. Censorship was more severe in the Lithuanian-speaking corner of the tsarist empire, and there was greater internal opposition to nationalistic ideas from the suspicious Catholic clergy and from the socialist movement. On the other hand, Lithuanian nationalism could draw on a more potent political history for the myth of national renaissance—there had been, after all, a Polish-Lithuanian state—and on a substantial and relatively wealthy émigré community in the United States, which the Estonians and Latvians did not have. As Senn points out, much of Basanavičius's scholarly work on Lithuanian history and folklore was superseded. In the final analysis, however, it is not so much his scholarship as his persistence in the face of what must have seemed at times impossible odds that entitles him to the designation of “patriarch” of the Lithuanian national movement.

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I. IA. FROIANOV. *Kievskaja Rus': Očerki sotsial'no-politicheskoj istorii* [Kiev Rus: Essays on Social and Political History]. Leningrad: Leningrad University Press. 1980. Pp. 256. 1 r. 50 k.

I. Ia. Froianov believes that Soviet scholars have been too optimistic in evaluating the degree to which the society of Kievan Rus can be described as feudal and that the transition from clan-tribal to feudal social relations was considerably slower than most studies postulate. He explored the significance of archaic clan-tribal elements in Kievan socioeconomic life in his previous monograph, *Kievskaja Rus': Očerki sotsial'no-ekonomicheskoi istorii* (1974). In his new book Froianov extends his revisionist analysis to the realm of sociopolitical relations, as well as addressing a problem he claims is overlooked in existing Soviet scholarship—the role of the masses (“the people,” *narod*) in Kievan politics.

Kievskaja Rus': Očerki sotsial'no-politicheskoj istorii consists of seven thematic chapters on the prince, the retinue (*družina*), the question of seigneurialism, the “men” (*liudi*), the popular assembly (*veche*), the “people” (*narod*) and military forces (*voisko*), and the significance of the city. Each chapter begins with a historiographic survey of imperial Russian and Soviet scholarship (only) and then presents its data chronologically; this organization necessitates both repetition and cross-referencing. Froianov expresses the largely fulfilled hope that his chapters are sufficiently integrated to constitute a coherent book, although this does not compensate for the absence of a summary concluding chapter.

Froianov argues that in the absence of large-scale “feudal” landholding by the princes, their retinue, or an aristocracy, political and military power was most often decisively exercised by the democratic *veche*, whose armed urban and rural freemen, the *liudi* or warriors (*voi*), served popular (*narodnyi*) interests. The apex of the sociopolitical structure was the city-state or city-land (*zemlia*), which continued to be influential throughout the Kievan period, although, as feudalism developed, clan-tribal democratic sociopolitical institutions gradually disintegrated and declined.

The notion of Kievan Rus as a transitional society is not novel, but Froianov's book contains many intriguing insights—for example, on the shifts from collecting tribute (*dan*) to taxation (as a “feeding” [*kormlenie*]) or from positive to negative immunities. Nevertheless, Froianov's bold conclusions rest upon an uneven foundation, and much interpretation of the written evidence strikes me as too imaginative. It should be remembered that Froianov operates within the orthodox Soviet Marxist conceptual framework of clan-tribal and feudal social formations as successive and antagonistic; an additional constraint is the standard Soviet defini-

tion of "feudalism." Not every reader will necessarily see all manifestations of customary or kinship relations as deriving from the clan-tribal system nor identify any and all social groups below the aristocracy as the "people." Although Froianov assays comparative analysis with all clan-tribal, prefeudal societies, I have never found the similarities of Kievan Rus and classical Greek city-states convincing, in either Froianov or Vernadsky, and other analogies are even more farfetched.

Still, this is a stimulating, scholarly monograph. Froianov's study of Kievan Rus is not yet complete, since he mentions future investigations of social structure per se and judicial procedures. It remains to be seen whether he can succeed in fashioning a new paradigm of Kievan society.

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ANDRZEJ WALICKI. *A History of Russian Thought from the Enlightenment to Marxism*. Translated by HILDA ANDREWS-RUSIECKA. Stanford: Stanford University Press. 1979. Pp. xvii, 456. \$25.00.

A professor of the Institute of Philosophy and Sociology of the Polish Academy of Sciences, Andrzej Walicki is already known to American and British academic circles. He is the author of two other works that have appeared in English, *The Slavophile Controversy: History of a Conservative Utopia in Nineteenth Century Russian Thought* (1975) and *The Controversy over Capitalism: Studies in the Social Philosophy of the Russian Populists* (1969). Moreover, Walicki has been a frequent lecturer in the United States and Great Britain and was a visiting professor at Stanford University. This experience has undoubtedly prompted him to make this English translation of his 1973 Polish work even more useful to English-speaking students by its frequent references to works in English.

Potential readers should not take this work as simply another noteworthy example of scholarship in a Communist-dominated and officially Marxist East European country; indeed, there is little here to indicate even a Polish, let alone a Marxist, provenance. Rather, this is a book that can and should be directly useful to American students of Russian intellectual and social history for its own intrinsic merits, all the more so as many past, standard surveys are becoming scarce and unavailable. To be sure, T. G. Masaryk's three-volume *Spirit of Russia*, V. V. Zenkovsky's two-volume *History of Russian Philosophy*, or N. O. Lossky's *History of Russian Philosophy* remain on many library shelves, but such works are by now dated and marked by particular biases—especially the last two, which reflect a conservative theological and metaphysical outlook.

Walicki's volume is up to date in its scholarship and well balanced. It also offers a well-constructed survey that manages to cover a great deal of ground in a single normal-sized volume. The work is very readable, thanks in part to the not merely competent but elegant translation by Hilda Andrews-Rusiecka.

Walicki has chosen to begin with the 1760s as the time when an independent Russian public opinion emerged along with the rise of the "intelligentsia," a group that he aptly describes as "a body of educated people who felt responsible for their country's future" (p. xv). The author attempts to justify his stopping at 1900, rather than 1917, by arguing mainly that political activism takes over the scene around 1900. However much this may be true, this reviewer regrets the author's decision to omit those interesting years before the 1917 revolution, which saw, for example, such intellectual reactions to the radical positivist and materialist traditions as the publication of *Vekhi* (*Signposts*) in 1909.

Nonetheless, within Walicki's eighteen chapters the reader will find evaluations of all the major figures of modern Russian intellectual history from Novikov and Radishchev to Plekhanov and the young Lenin. He will also encounter some decidedly less familiar figures, from such eighteenth-century pioneers of the Enlightenment as Iakov Kozelskii, Dmitrii Anichkov, and Semen Desnitskii to late nineteenth-century positivists such as Grigory Wyruboff, Vladimir Lesevich, or Matvei Troitskii. Walicki's training and special competence in the history of philosophy have caused him to deal more with certain philosophical problems than has been customary in most Western accounts of Russian intellectual history. He also does more with the thought of Dostoevskii and Tolstoi than scholars who, particularly if they do not admire either writer as an intellectual, prefer to leave them to the literary historians.

Walicki's survey is a rewarding one and deserves to be read by all serious students of the development of modern Russian thought.

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ROBERT EDELMAN. *Gentry Politics on the Eve of the Russian Revolution: The Nationalist Party, 1907-1917*. New Brunswick, N.J.: Rutgers University Press. 1980. Pp. xvii, 252. \$19.75.

Robert Edelman's study of the Nationalist party fills a void in our knowledge of politics in the late tsarist period. Edelman's major thesis is that the Nationalists, fully organized in 1909 and possessing a conservative ideology, were the only modern po-

litical party to emerge from the Duma experiment. The party's chief source of strength was the western Russian nobility. In one of the most interesting sections of the book Edelman theorizes that the insecurity and concerns of this group prompted them to organize and lobby, that lack of altruistic *zemstvo* service precluded scruples about power politics, and that familiarity with progressive agricultural methods made western Russian nobles receptive to modern political organization. In delineating the Nationalists, Edelman frequently compares them with the Octobrists in terms of political outlook, size of landholdings and education, and socioeconomic and educational levels of electors. The Nationalists' chief aim seems to have been the establishment of elective *zemstvos*, which they intended to dominate, in the six southwestern provinces. Edelman examines this campaign, analyzes the subsequent *zemstvo* and Fourth Duma elections, and depicts the Nationalists' deterioration during World War I.

Edelman's profile of the party—drawn from western Russian newspapers such as the *Kievlianin*, electoral data, and biographies of Nationalist leaders—is insightful and informative. There are, however, some untied threads. Edelman notes but does not satisfactorily account for the strength of the Nationalists in nonwestern provinces. Although he mentions the importance of the clergy to the Nationalists, he leaves this subject dangling. Also disquieting is Edelman's analysis of the linkage between Stolypin and the Nationalists, particularly his explanation for Stolypin's actions during the *zemstvo* crisis of 1911. Edelman hypothesizes that Stolypin committed political suicide during the crisis because he viewed the Nationalists as the new force in the Third Duma and determined they would be his power base in the Fourth. The Nationalists were a bloc to be reckoned with, but Stolypin also acted independently of them. For example, Edelman admits that the Nationalists were not interested in other aspects of Stolypin's so-called national program, such as the formation of Kholm province and the measures for Finland—policies that were very important to Stolypin and upon which he expended much energy. At one point, Edelman concedes that ultimately Stolypin's position did not derive from the Octobrists but from the tsar and that no amount of support from the former would have saved him if support from the latter were lacking. Why, then, would Stolypin risk Nicholas's wrath in 1911 to build a power base on the Nationalists for the Fourth Duma? Finally, in emphasizing the importance of the western *zemstvos*, Edelman casts Stolypin as a one-issue politician, whereas in reality he was preoccupied with a multitude of concerns.

Nevertheless, Edelman's explanation of the crisis is creative and his larger aim—dissecting a con-

servative group that was politically streetwise—is fascinating and thought provoking.

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G. L. SOBOLEV. *Oktiabr'skaia revoliutsiia v amerikanskoi istoriografii 1917–1970-e gody* [The October Revolution in American Historiography, 1917–70]. Leningrad: Nauka. 1979. Pp. 247. 2 r. 10 k.

By the mid-1930s Soviet historians writing about the October Revolution arrived at a relatively standardized conception of why the Bolsheviks were successful in 1917. Except for the demythologizing of Stalin's role in the revolution, Soviet historiography has remained consistent in its insistence on the following points: (1) the Bolshevik victory was historically inevitable, and it occurred in conformity with historical laws; (2) the Russian Revolution was not a purely Russian phenomenon but was an event with great international significance; (3) the Bolshevik victory was a popular movement, not a conspiracy or *coup d'état*; and (4) the revolution was truly a socialist revolution. In his review of American historiography of the October Revolution, G. L. Sobolev shows how the "progressive" writers have accepted the Soviet interpretations, while the bourgeois historians attempt "to distort the true character of the October Revolution and to underestimate the influence of its ideas on the world revolutionary process."

The most interesting section of Sobolev's study is found in the fifth chapter where he examines the attempts by American historians of the 1960s and 1970s to find a "new approach" to the history of the October Revolution. More than fifty pages are devoted to the review of recent American interpretations of the revolution, including journal articles and book reviews. Although Sobolev concludes that American historians continue to distort Russian reality, he does present to his Soviet readers in some detail the ideas of such historians as Theodore Von Laue, Adam Ulam, Robert Tucker, Robert Daniels, and Alexander Rabinowitch. He is also rather specific in presenting the different approaches of the contending historiographical schools.

Those who are familiar with the attempts of Soviet historiography to discredit non-Soviet views on Russian history will find little that is new in this book in terms of interpretation. Sobolev, however, does do a good job in summarizing the Soviet view of American sovietology and the American writings on the Russian Revolution. His book is also a valuable source for finding articles in Soviet periodicals and more detailed book reviews by Soviet scholars

on American historiography. For these reasons American sovietologists may find this volume interesting to skim, but few will feel it necessary to read it from cover to cover.

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JAMES H. BATER. *The Soviet City: Ideal and Reality* (Explorations in Urban Analysis, number 2.) Beverly Hills, Calif.: Sage Publications. 1980. Pp. xii, 196. Cloth \$18.95, paper \$8.65.

A socialist autocracy that controls and owns the economy and the social infrastructure of society and that has decided to make as one of its priorities the improvement of the overall welfare of the population would seem to provide the most advantageous conditions for the rational planning of urban development. Such favorable conditions have existed in the Soviet Union since 1953 when Stalin's successors shifted priorities from sacrificing the immediate welfare benefits of socialism in favor of developing the industrial and security base of the country to making welfare and particularly the improvement of urban living a major priority of the system. The question is: how well has this campaign to create the ideal socialist urban environment fared over the last two and a half decades? Have the socialist conditions for city planning simplified urban development? *The Soviet City: Ideal and Reality*, the second volume in the series "Exploration in Urban Analysis," seeks to answer these questions.

Beginning with a short history of urban development from Peter the Great through the uncontrolled overcrowding of cities in the 1900s caused by rapid industrialization to the neglect of and minimal attention to city planning under Stalin, James H. Bater examines the problems of city planning in Russia. From his discussion it is clear that the difficulties faced by Soviet urban leaders and planners in some ways differ from those in other countries, but they are no less formidable. Although the major problem has been to overcome the acute shortage of housing and lack of adequate planning in the past, other difficulties in the Soviet system have obstructed rational planning. These included the shortage of qualified city planners; the slow pace at which plans were developed and approved (often resulting in plans being out of date before they were implemented); the unwillingness of industrial enterprises and other organizations to turn over their housing and consumer services to local governments as ordered by Moscow; the refusal of industries to abide by the city plans in building and expanding their plants and adhering to envi-

ronmental controls; the limited financial resources of local governments; the continued rapid increase of the urban population; the inability to expand consumer services to keep pace with housing developments; and the difficulties in maintaining quality controls over construction. Bater reviews these and other problems in detail from a planner's and geographer's point of view. The political and economic aspects of the problems are dealt with only briefly. The volume is written as a text for undergraduates to acquaint them with "some facets of Soviet urbanization experience," and Bater states that neither his analysis nor conclusions are original. Nevertheless, he has gathered a wealth of additional evidence about the dimensions of the problems and about the frankness with which the Soviet press and periodical literature have dealt with the issues. He has also been careful in making his analyses and has not overlooked the tremendous strides that have been made in planning and improving the quality of Soviet urban life over the last twenty-five years. Except for its pedantic style, this volume is an excellent survey of the attempts by Soviet planners to provide rational guidance to the development of Soviet cities.

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R. W. DAVIES. *The Industrialisation of Soviet Russia*. Volume 1, *The Socialist Offensive: The Collectivisation of Soviet Agriculture, 1929-1930*; volume 2, *The Soviet Collective Farm, 1929-1930*. Cambridge: Harvard University Press. 1980. Pp. xxi, 491; x, 216. \$35.00; \$18.50.

These are the first two volumes of R. W. Davies's projected five- or six-volume study of the industrialization of Soviet Russia. Rightly, the beginning volumes are devoted to the introduction of collectivization. Once at the pinnacle of Soviet power, Stalin set a course for industrializing the union as rapidly as possible. Achieving that end demanded a massive infusion into the cities of both capital and manpower that could be found only in the countryside.

For Marxist-Leninists the method of capturing agriculture's resources was clear. If control over the rural means of production could be secured, all else would fall into place. Most importantly, control over the farms would secure not only the grain necessary to feed the industrializing cities but also the additional quantities needed for export in order to finance the imports required for industrial construction. Forced collectivization was the answer, which also resulted in a large-scale movement of manpower from the farms to the

cities, providing additional millions of workers to operate the new factories.

Unfortunately for the leaders' plans, peasant attitudes proved to be an enormous obstacle. Attempts to persuade the peasantry voluntarily to accept collectivization were almost a total failure. In June 1927 "only 0.8% of all the peasant households in the U.S.S.R. had joined collectives" (vol. 1, p. 109).

In spite of the overwhelming rejection of collectivization, divide-and-rule tactics achieved the end. The trap of collectivization was set, and the peasant *kulak* (literally, tight fist) was used as bait. In theory the *kulaks* were the well-to-do peasants who exploited the labor of their poor neighbors. In practice, however, the drive to root out the *kulaks* also netted anyone who "resisted official policy"—in short, any peasant who rejected the new "ideology" (vol. 1, pp. 98, 231). "Tens of thousands of peasant families," who were poor by any definition, were caught in the drive.

The party and the army assisted the operation, but the major group directing activities on the rural scene was the 250,000 urban "plenipotentiaries" sent into the countryside to organize the peasants, particularly the poor and disaffected villagers. Not only did most such peasants have nothing to lose, but they also often used the drive to settle old scores with hated neighbors. Often the voters who showed up at the village *skhod* (general meeting) were dominated by the "poor" who would force the issue of collectivization.

One additional aspect of the scheme was vital to its success. Had the peasants been able to hang on to their implements and especially their draft animals, they probably would have been able to resist collectivization. Stalin realized that the domination of the farms by the Machine-Tractor Stations (first established in 1927 on a voluntary basis) was vital to his plan. Not only was the scarce machinery taken from the peasants, but their draft animals were taken as well. As a result, the holdouts could not plant the next crop. Failing to join the collective meant starvation. Still, the peasants had their livestock, which they slaughtered (rather than turn them over to the collectives that they themselves were entering) in such numbers that, whereas there had been 70.4 million head of cattle in July 1928, the number fell to only 52.5 million two years later. Indeed, the number of cattle in the USSR did not again reach the 1928 level until the mid-1950s.

The two most crucial years in Soviet peacetime history have been 1929–30. Davies's account is by far the most comprehensive treatment we have seen of the calamity that was visited on the Russian countryside during those two years. The

work took enormous effort, and the documentation is extensive. The tables in the appendixes contain much statistical data that have never before been printed in the West. His work greatly advances our knowledge of Soviet history. Yet we find the volumes particularly lacking in analytical perspective on important counts.

Almost totally lacking is an appreciation that introduction of the collectives and the MTS provided a base not only for controlling the economic side of agriculture but also for corralling the peasants politically and socially. Most serious is a failure to appreciate the enormous long-term cost of wholesale attempts to transfer industrial organizational forms to farming. For example, without commentary Davies cites an observation by Markovich (then the head of *Traktorsentr*) that "there is no difference in principle between agriculture and urban industry" (vol. 2, p. 46). After more than a quarter-century attempting to analyze the human side of agricultural problems in the USSR, the US, and the developing nations, we have come to the conclusion that one of the most serious causes for faulty agricultural policies is the widespread misconception by political leaders that there is no essential difference between what works in agriculture and what works in industry. The factory of farming is the whole outdoors. Except for a few specialized operations, such as poultry raising, that may be enhanced by industrial methods, the successful nurturing of living plants and animals requires techniques, approaches, and attitudes quite unlike the regularized setting of regimented labor in industry.

Lenin was really speaking of the road to agricultural industrialization when he asserted that "communism is the electrification of the countryside." Repeatedly championed, that view has never been challenged by his successors. Yet collectivization has meant the imposition of bureaucratic form upon the rural USSR, an unwieldy handicap that has cost the Soviet Union dearly and that still plagues Soviet agriculture at the beginning of the 1980s.

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NEAR EAST

STEVEN T. ROSENTHAL. *The Politics of Dependency: Urban Reform in Istanbul*. (Contributions in Comparative Colonial Studies, number 3.) Westport, Conn.: Greenwood Press, 1980. Pp. xxix, 220. \$27.50.

This is a study of an Ottoman attempt to organize modern administrative institutions in one part of Istanbul, the European and Levantine district known as Galata. These institutions were to provide a

model for later reorganization of the entire metropolis. Steven T. Rosenthal distinguishes two periods in the Galata experiment, one of leadership by non-Muslim bourgeois (1858–63) and one when the Ottoman government took matters under closer control, introducing officials into the district council and achieving better results (1863–67).

Rosenthal draws on dependency theory to show how European interests hindered reform, particularly during the first period. Before 1863, the Galata council concentrated on projects of commercial significance, of benefit primarily to individuals like those who dominated the council, but the council failed in tax collection and went bankrupt. The European powers, whose embassies were located in the district, resisted some initiatives of the council, while the capitulations impeded its work in that numerous residents of the district, holding foreign passports, were exempt from many Ottoman taxes and from prosecution in Ottoman courts. After 1863, the substitution of skilled Ottoman administrators in the leadership of the council, the expansion of the public works program into projects of general benefit, and persistent efforts to raise revenue and maintain order produced clear improvement. This success hastened the generalization of the new administrative model throughout Istanbul.

Rosenthal's presentation is generally persuasive, although not equally satisfying in every respect. The sources come mainly from British embassy papers, contemporary French newspapers of Istanbul, some Ottoman archival files, and the first volume of Osman Nuri Ergin's five-volume work on urban affairs. Rosenthal does not explain why he does not use other volumes of this last work, some of which appear to contain relevant materials; and it is disappointing that his Ottoman documentation is not more extensive. The handling of names and concepts in original languages is disquieting at times. To cite only a few examples, Fuad Paşa's "colleague" (p. 64, n. 20) is not Mehmed Ali Paşa, but the grand vezir Mehmet Emin Âli Paşa; the telegraph director, Esbak Mahmud Efendi (p. 148), is almost surely the former (*esbak*) telegraph director, Mahmud Efendi; the often-mentioned Taxim should be Taksim. The focus of the work, finally, is narrow. It deals mainly with one district of the Ottoman capital during the years 1858–67 and makes only limited reference to the rest of Istanbul or other Ottoman cities.

Aside from its general theoretical or comparative interests, Rosenthal's work will appeal mainly to students of Ottoman local administration. His conclusions reinforce the growing consensus in that field concerning the gradual administrative improvements of the reform era.

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CHRISTOPHER J. WALKER. *Armenia: The Survival of a Nation*. New York: St. Martin's Press. 1980. Pp. 446. \$30.00.

Most histories of Armenia written in Western languages have concentrated on periods before the fall of the last Armenian kingdom in the fourteenth century. The subsequent "dark centuries" of alien, primarily Turkic, domination tend to be treated cursorily if at all. In recent years, specific aspects of those troubled times have been investigated in monographs, but a synthesis of the period as a whole has not been forthcoming. Christopher J. Walker has attempted that approach, at least for the nineteenth and twentieth centuries, and has generally succeeded as far as political history is concerned.

Using a combination of archival materials, published diplomatic correspondence, travel literature, scholarly studies, and popular accounts, Walker presents the Armenian experience under the tsars (part 1) and under the sultans (part 2) and compares the two. As it pertains to Western (Turkish) Armenia, it is mostly a record of woeful oppression, misplaced hopes for reform, harmful European involvement, deepening Turkish racism, and repeated betrayals and massacres of the Armenians, culminating in the genocide that began in 1915.

Walker levels a sharp indictment against the callous imperialism and political immorality of the European powers and particularly against the dominant British aristocratic class that shaped foreign policy. The study emphasizes the British role in the Armenian tragedy, in view of the repeated British attempts to salvage the corrupt and decaying Ottoman empire through inadequate, partial reforms. Walker expresses dismay that there are still scholars and politicians who deny the plan of the Turkish Committee of Union and Progress to annihilate the Armenian people. His vivid style and use of many excerpts from primary sources create strong images and intense scenes in chapters with titles such as "No Help Came" and "The Death of Turkish Armenia."

The study includes a long, informative chapter on Armenian attempts to establish a republic after the holocaust of 1915–18 and the domestic and international factors that prevented the fulfillment of those strivings. Walker regards the ultimate boundary drawn between the new Turkey and the new Soviet Armenian state in 1921 as grossly unjust yet nonetheless a relief in that it ended the years of indecision and allowed the surviving Armenians to begin the process of rebuilding on the little territory left to them under Russian protection. The final chapter on the Armenians since 1921, treated as an epilogue, is sketchy and sometimes misleading. The volume concludes with a helpful biographical sec-

tion on prominent Armenian political, civic, and religious figures.

Walker has not attempted to deal in significant detail with socioeconomic, cultural, or intellectual history. For readers accustomed to scholarly writing, his sharp, barbed style may be detractive, and his occasional simplified explanations may draw attention from the basic soundness of the research and organization. There are a number of inconsistencies in transliteration (for example, Armen Karo, p. 198; Armen Garo, p. 382).

The volume is suitable for survey classes of Armenian and Near Eastern history in conjunction with other texts and will surely appeal to general readers wishing to gain a broad overview of Armenian history during the last two centuries.

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SHAUL MISHAL. *West Bank/East Bank: The Palestinians in Jordan, 1949–1967*. New Haven: Yale University Press. 1978. Pp. xiii, 129. \$9.50.

The story of the Palestinian Arabs during the two decades that followed the end of the British mandate has received little attention. The Palestine crisis seemed to most to be one between Israel and the neighboring Arab countries, with the Palestinian Arabs constituting part of the Jordanian entity. Shaul Mishal deals with that crisis from a new angle: he addresses the transformation of the Palestinian Arab community within the Jordanian regime; he attempts to characterize the changes in the identity of the Palestinian Arabs; he tries to examine the degree of integration between residents of the East and West Banks and explore the effect of the neighboring Arab countries on the process of that integration.

Mishal traces, in six chapters, the development of Palestinian political identity. In the first three, he gives a very concise introduction to the Palestinian Arab community and then examines the relations between the Palestinians and the Jordanian government during the latter's attempt to develop a concept of legitimacy for the new kingdom. The second half of the book deals with the growth of a Palestinian identity aided by political factors seeping from the other Arab countries, such as Nasserism and Bathism. Mishal subscribes to the view that the Jordanian authorities manipulated the political process to produce cooperation and acceptance by the Palestinian Arabs and thus secure the support of some elements of the Palestinian leadership in order to create legitimacy for the new regime.

In developing his thesis, Mishal makes no reference to the role played by the army in preserving

the regime. It was the recourse to military measures that saved the system time after time. In addition, in dealing with the concept of "legitimacy and concession" during the Nabulsi premiership, Mishal considers the government of Nabulsi as part of the concession. The struggle seems to him to lie between the king and his supporters and the Palestinian Arabs. He does not take into consideration the part played by original trans-Jordanian elements. Many of the opponents of the regime were residents of the East Bank; Nabulsi belonged to that group and so did the leaders of the three coup attempts between 1958 and 1960. Mishal, as well, does not examine the part played by Palestinian Arab elements of the East Bank, although the title of the book suggests the necessity of that examination.

Mishal's contribution, however, is considerable. He produced a book that deals with a topic not frequently tackled and thus gives an insight into the development of Palestinian Arab identity. He drew material for the book from new sources made available to Israeli scholars after the Six Day War.

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AFRICA

JOHN TALBOTT. *The War Without a Name: France in Algeria, 1954–1962*. New York: Alfred A. Knopf. 1980. Pp. xii, 305. \$12.95.

Thanks to a plentiful outpouring of memoirs and anecdotal chronologies, we know a good deal today about the French reaction to the Algerian revolution, which lasted from November 1954 until July 1962. (On the Algerian side, by contrast, the record is far less complete, and our ignorance about many basic historical aspects of the revolution is correspondingly much greater.) Much of the interpretive work on French policy nonetheless remains to be done. What special importance should we assign to the Gaullist vision of postwar France, for example, and the role this played in setting French policy on a collision course with Indochinese and Algerian nationalism after 1945? Amid praise for the General's rescue of democratic government in 1958 and his liquidation of the Algerian problem in the following years, it is often forgotten that his "certaine idée de la France" was not without its own responsibility for the trials of French decolonization in the first place. Or should we rather seek to explain French policy in Algeria by looking to the ideological bankruptcy of the Socialist party (SFIO), which could provide no counterweight to conservative opinion? Whether we look at the party's conduct

with respect to the Statute of Algeria in 1947, or to its collaboration in the sabotage of constitutional liberties in these "three North African departments" thereafter, or to the utter failure of Guy Mollet's regime (the longest and most critical of the Fourth Republic, lasting from January 1956 until May 1957) to comprehend the character of the revolution, we are confronted with a systematic and deep-seated refusal to face up to the realities of the times with respect to Algerian nationalism.

Although questions such as these are central to the debate on how to understand the French response to pressures for decolonization, any number of other issues arise as well, such as the structure of French politics and the nature of settler relations with France, or the attitudes of business leaders and technocrats determined to carve a more dynamic economic future for France. Addressing these matters is all the more inviting for those of us who are not French since, as in other moments of national division, the French are liable to avoid the discussion themselves.

In circumstances such as these, where interpretive arguments on a variety of topics wait to be advanced, it is all the more disappointing that John Talbott's book on the French role in this war should be so superficial. Its short length does allow us, to be sure, what is perhaps the most readable, concise account of the French reaction to the revolution to be found in English. Talbott's style is vivid, his account covers the major highlights, and his explanations are orthodox (although Alistair Horne's *Savage War of Peace* [1978] is far more comprehensive, even if it too skirts many of the analytical issues). The book's shortcomings are simply that it tells us nothing that is new, and it makes no effort whatsoever to engage any of the interpretive issues that might have been raised. According to Talbott, the book was written over a period of at least ten years with the support of numerous scholarly institutions on both sides of this continent as well as on both sides of the Atlantic. We might have expected something more substantial.

TONY SMITH
Tufts University

TOM W. SHICK. *Behold the Promised Land: A History of Afro-American Settler Society in Nineteenth-Century Liberia*. (Johns Hopkins Studies in Atlantic History and Culture.) Baltimore: Johns Hopkins University Press. 1980. Pp. xv, 208. \$18.00.

Edward Blyden had a vision of Liberia's role in world affairs. It was to be a symbol of the Africans' capacity for independence, an inspiration to oppressed American blacks, and the instrument for African regeneration. Unfortunately Liberia came

to symbolize something else. As early as 1817 a free black convention denounced Liberian colonization as a conspiracy and an insult, a theme reiterated by David Walker's *Appeal*, the convention movement of the 1830s, Frederick Douglass, and most other black American leaders. Even Martin R. Delany, who approved the establishment of an African homeland, rejected Liberia in favor of the Niger Valley. Black fugitives in Canada resisted a proposed visit by Harriet Beecher Stowe in 1856 because *Uncle Tom's Cabin* concluded with an apparent endorsement of Liberia. Liberia's subsequent financial incapacity, the League of Nations allegations concerning the treatment of indigenous Africans, and the stories of political corruption made Liberia an embarrassment to most black Americans and to many Africans. No one wanted to be judged by that standard.

Whether Liberia deserved this negative image or not, it surely helps to explain its failure to attract more American immigrants and, in turn, the trend toward an increasingly isolated and inward-looking settler society that even Blyden would eventually flee. Tom W. Shick largely ignores this context for Liberian development. Instead, he accepts Blyden's earlier hope as a reality and has Liberia "representing the Promised Land for people of African descent in the New World." Despite this dubious premise, the book is successful in treating many external issues and attitudes against which the Liberian experiment had to contend. Some of its most valuable sections deal with the problems of imperial encroachment and the attempt to establish a viable economy in the face of contrary global forces. The chapter describing Lincoln's negotiations to relocate American freedmen in the West Indies offers an important insight into the conditions confronting black Americans after the Civil War.

This book's chief purpose is to explain the social dynamics of settler society. Through several intimate family studies, Shick supplies evidence revealing that the settlers themselves were consciously attempting to build a new promised land, deliberately preserving an American heritage while adapting to African reality. But there is inadequate support to show that the case studies reflect the general society. An "elite" is mentioned but never defined, nor is its membership delineated. Tables offer the founding dates of benevolent, literary, and fraternal societies, with no hint of the numbers enrolled to illustrate popularity or influence. Even population totals are neglected, except for an analysis of the 1843 census, although published figures are available on immigrants throughout this period. The introductory promise to employ quantitative data is rather selectively fulfilled.

Shick does contribute to an understanding of some significant external factors, useful from the

black American perspective as well as the Liberian, and he portrays the "social cement" of family ties in Liberian cultural evolution. But these themes are not satisfactorily integrated to present a convincing impression of how and why Liberia produced a unique Afro-American civilization.

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I. K. SUNDIATA. *Black Scandal: America and the Liberian Labor Crisis, 1929-1936*. Philadelphia: Institute for the Study of Human Issues. 1980. Pp. ix, 230.

On April 12, 1980, the 139-year rule of the True Whig party in Liberia was ended by a violent coup. The party had been an instrument that the Americo-Liberians, descendants of freed American slaves who had colonized Liberia's coastal areas in the early 1800s, used to preserve their political and economic control of the country. Its political activities and economic aims had reflected those of a significant minority of the population: a group consisting of approximately 3 percent of the nation's 1.7 million inhabitants.

The grievances underlying the coup—corruption, violations of human rights, and policies detrimental to the majority of Liberians—had been the basis for complaints against the ruling Americo-Liberian elite on numerous occasions in the past, and no one familiar with the history of this West African state was totally surprised when President William Tolbert's regime was toppled by junior military officers identifying with the aggrieved indigenous masses. Indeed, as Ibrahim K. Sundiata demonstrates so persuasively in *Black Scandal*, the situation existing in Liberia on the eve of the coup was a direct outgrowth of the play of events in the period between 1929 and 1936. It stemmed from the unwillingness of the elite to alter basic power relationships in any significant way at a time when modification was not only called for but feasible.

In June 1929, the United States Department of State accused the Liberian government of direct involvement in a system of labor export to Fernando Po "hardly distinguishable from [the] organized slave trade" (p. 1). American concern, Sundiata argues, although packaged in the rhetoric of humanitarianism, was in large part motivated by the desire to protect the investment of American capital in Liberia, especially the interests of the Firestone Rubber Company. Under international pressure, Liberian officials agreed to a League of Nations inquiry into labor conditions in their country—an investigation that revealed that indigenous Liberian workers, mostly from the Kru and Grebo peoples, were being forcibly recruited and exported to work on foreign cocoa plantations, and otherwise ex-

ploited by officials of the Americo-Liberian elite who profited handsomely from these transactions.

The findings of the League investigators, according to Sundiata, "became part of a larger debate on the fitness of blacks for self-determination" (p. 1). For some, confirmation of the scandal was also confirmation of their long-held belief that Africa's sole independent black republic had failed as a nation. For others, more blatantly racist, the scandal was proof of black incapacity, and they prescribed "foreign" (meaning "white") control over the Liberian government. Black American opinion was divided on the specific nature of the medicine that might be prescribed to cure Liberia's ills but consistently viewed that state as a symbol of black independence and as "the hope of the diaspora."

The Liberian national elite survived the crisis and the attacks through skillful manipulation of internal and external sentiments and forces. Despite the fact that meaningful reforms and a redistribution of power within Liberia might have been initiated at this juncture, the elite met the threat of foreign interference by drawing on prevailing anti-imperialist sentiments in Europe and the United States. It cloaked itself in the doctrine of national sovereignty, steadfastly maintaining that there could be no interference in Liberia's internal affairs. It took advantage of the inability of many concerned black Americans and Pan-Africanists to fathom the contradictions in the Liberian situation. It succeeded in appeasing world public opinion by enacting reforms that prohibited labor export and the pawning of human beings. At the same time, it settled into a symbiotic relationship with Firestone and other foreign interests and opened the door for large-scale labor exploitation within Liberia itself. Most important of all, it managed to strengthen its position at home vis-à-vis the indigenous majority by clamping down on dissent and crushing internal opposition.

Sundiata's narration and analysis of these events, based on research in Liberia, the United States, Great Britain, Spain, and Equatorial Guinea, is both thorough and effective. His book is not only an important contribution to the history of Liberia and United States-Liberian relations but also to the study of Afro-American thought about Africa and the evolution of Pan-Africanism. *Black Scandal* tells a sad yet fascinating story and provides crucial background for contemporary events.

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GWYN PRINS. *The Hidden Hippopotamus: Reappraisal in African History; The Early Colonial Experience in Western Zambia*. (African Studies Series, number 28.) New York: Cambridge University Press. 1980. Pp. xvi, 319. \$45.00.

Gwyn Prins seeks to reappraise the history of Buloz (Barotseland) during the crucial years 1876–96 when the British and the Lozi first established contact. His work is based on a thorough perusal of primary sources available in London, Lusaka, and Salisbury, as well as on two years' residence in Buloz. Prins seeks to depart from conventional Eurocentric historiography. In so doing, he departs from conventional narrative techniques. He attempts to look for specific "core areas" whose elucidation will throw new light on his subject. He takes issue with traditional historians who, he believes, underestimated the success attained by the Lozi in dealing with the whites. The author tries to write "total history," covering every aspect of life. He believes that his extensive field notes and archival sources must be used in a process of reciprocal control, which will reveal the limitations inherent in both and provide the reader with the means of evaluating the author's interpretation.

The author's erudition is impressive. His work invites criticism, however, on two counts. To Prins, as to most representatives of the "total history" school, all facts are equal in the sight of history. This approach lends to a cluttered narrative that may make an understanding of the subject more, rather than less, difficult. An ethnographer, no doubt, is justified in providing the reader with the minutiae concerning, say, the building methods of a rural people. But does a student interested in Anglo-Lozi relations during the last two decades of the nineteenth century really have to know the refinements of each building style used for the construction of a Lozi hut?

Secondly, the author claims too much credit for his method. In his own estimation, Prins avoids the "fundamental conceptual and methodological flaws" that beset yesteryear's hidebound historiography. To the author, it comes as a startling insight that the Lozi, when faced with the European peril, behaved like the hippopotamus of Lozi folklore. When threatened with danger, the majestic beast retreated into deep waters. The Lozi adopted a similar strategy. By doing so, they emerged from the early colonial experience with considerable success, their state and self-image intact. These conclusions do not seem startling. L. H. Gann's pioneering work, *History of Northern Rhodesia* (published sixteen years ago and surprisingly unmentioned by Prins either in his text or his bibliography), arrived at a similar evaluation. According to Gann, the treaty system had benefited the Lozi king. The monarch managed to mobilize British support against both his internal and his external enemies—white and black. The Victoria Falls agreement, an important document in the series, was, according to Gann, "almost certainly the best that the Lozi could obtain. . . . The aristocratic structure of their

kingdom remained unchanged. . . . They managed to keep the core of their kingdom inviolate from permanent European settlement." But, in the long run, colonialism led to profound societal changes that the treaty makers could not anticipate. So much for the conclusions of a supposedly discredited scholarship!

Despite his deficiencies, the author—with his ebullient self-confidence and his capacity for hard work—shows distinct promise. One hopes that he will avoid the weaknesses of the present work in the second volume yet to come.

PETER DUIGNAN
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D. M. SCHREUDER. *The Scramble for Southern Africa, 1877–1895: The Politics of Partition Reappraised*. (Cambridge Commonwealth Series.) New York: Cambridge University Press. 1980. Pp. xiii, 384. \$29.50.

D. M. Schreuder has already in his *Gladstone and Kruger* (1969) analyzed the period of South African history from 1880 to 1885. He has now written an "attempt at synthesis and reappraisal" (p. xi), utilizing much recent research as well as his own reassessment of primary sources, covering the period from the annexation of the Transvaal to the assumption of British control over Thongaland early in 1895. The detailed narrative therefore closes on the eve of the Jameson raid, with the annexation of the last independent African territories, but there is a concluding chapter that analyzes the increasing rivalry of the victorious white colonial states down to the outbreak of war in 1899.

Altogether this is a most welcome addition to the growing body of writing reassessing the scramble for Africa. Despite an intriguing reference to family oral tradition about the origins of an Afrikaner sense of collective identity against "perfidious Albion" (p. 51), Schreuder's scholarship is notably objective. The basic theme of the book is that British jurisdiction in southern Africa expanded at the price of Whitehall and Westminster devolving authority to the sub-imperialism of settler colonialism. In the jargon of the day, "the colonial factor" superseded "the imperial factor."

The archetypal colonial figures were Cecil Rhodes and Sir Hercules Robinson, high commissioner and governor general at the Cape. Schreuder's account of their maneuvers to procure a royal charter conferring almost unlimited power on Rhodes's British South African Company is detailed and revealing. But this was simply one key episode in a general process of imperial abdication that undermined commitment to trusteeship on behalf of the "natives." The main reason for this, as Schreuder repeatedly documents, was that the colo-

nial factor was considered to be much more economical than the imperial factor and saved the British taxpayers' money. Rationalizing after the event, British officialdom surrendered the intellectual basis for trusteeship by holding that British colonists "understood Native Management much better than we do" (p. 269). The missionary-humanitarian lobby also split. (If Schreuder's research had carried him into mission archives he could have shown how Rhodes adroitly manipulated missionary opinion by offering African land for operations in Rhodesia.) In addition to extensive footnotes, there is a useful bibliographical essay where Schreuder's comments on other historians are always generous and often astute. Such irenic historiography makes a refreshing change from recent ideological partisanship.

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A. N. PORTER. *The Origins of the South African War: Joseph Chamberlain and the Diplomacy of Imperialism, 1895-99*. New York: St. Martin's Press. 1980. Pp. xiv, 321. \$22.50.

This welcome monograph adds new dimensions to the discussion of official policy leading up to the Boer War. A. N. Porter has used the Foreign Office records to reveal the anxiety with which Lord Salisbury viewed the clandestine operations of those German and French financial and commercial agencies in Mozambique working for an administration of Delagoa Bay favorable to the Transvaal. He has set the diplomatic dialogue between the Colonial Office and the Transvaal firmly in its context of British public opinion and government policy. He has also done much to disentangle the separate ploys of Milner and Chamberlain, and the different viewpoints of the Colonial Office officials, including the undersecretary, Lord Selborne. Selborne emerges as much more on Milner's side than Chamberlain's, while Chamberlain, unaware of the extent to which Milner manipulated the South African press, became entrapped in a showdown with Kruger on Milner's terms. While Selborne and Milner wanted war, Chamberlain evidently did not.

Much space is devoted to the anatomy of public opinion and to Chamberlain's popularizing methods. While the Foreign Office kept secret its fears of German penetration—the Anglo-German convention of 1898 was "a holding operation in which Salisbury realised he was playing rather desperately for time" (p. 159)—the colonial secretary was allowed to work up public indignation over the maltreatment of the *uitlanders*. For Chamberlain this was an opportunity to educate the public in its imperial responsibilities, while

also building up the overt public pressure needed to convince Kruger that the government meant to have reform. Along these lines Porter challenges the assumption of R. E. Robinson and J. Gallagher that Chamberlain's diplomacy was little influenced by domestic opinion. He does not directly challenge their view of Chamberlain as hoping to reconstruct South Africa "as a unit of defence, and perhaps also of trade, under a central federal authority in London" (*Africa and the Victorians*, p. 439), but it is significant that in his detailed coverage of Chamberlain's policies there is nothing to support any such view. A federated South Africa was a different proposition from a federal empire, and Chamberlain only came out boldly for the former after the war had begun.

The emphasis of this book is on the antecedents of popular diplomacy, going back to Canning and Palmerston, and on the detailed process of telegrams and dispatches in rapport with opinion. The differences in political ideals separating Chamberlain the believer in democracy and "natural" progress from Milner the antidemocrat and apologist for the state only appear through the seams of close textual argument. As affecting the "will to war," they were probably of prime importance.

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ASIA AND THE EAST

JUDITH A. BERLING. *The Syncretic Religion of Lin Chao-en*. (Neo-Confucian Studies or Buddhist Studies and Translations, IASWR series.) New York: Columbia University Press. 1980. Pp. xv, 348. \$20.00.

The present work takes the doctrines of Lin Chao-en (1517-98) as a case study in Chinese religious syncretism. Judith A. Berling takes Western sinologists to task for having characterized the formation of Chinese religion as a retrograde process of mindless eclecticism that only corrupted the participating traditions of Confucianism, Buddhism, and Taoism. Syncretism, as a selective process rationally serving clearly defined religious ends, is here sharply distinguished from mere eclecticism.

The main thesis is that Lin Chao-en, trained in his youth for a bureaucratic career, was driven by a spiritual crisis to transcend the Confucian tradition in its then current orthodox forms. Anchored in the essentially Confucian belief in a moral universe and in the concept of sagehood as a state of moral perfection, he radically reinterpreted the Taoist inner alchemy as a method of moral self-cultivation. To a relatively minor degree, he also adapted Buddhist practices as aids to meditation.

Lin Chao-en authenticated his method by demonstrating its power to heal physical disorders and by his claim to have received it from a shadowy "enlightened master" who may have appeared to him in a dream. He founded a school that he made respectable in the eyes of the officials by the severity of its discipline, the vows of service taken by master and disciples, and its inclusion of Confucian classical learning in its curriculum. The *I Ching* served as the main canonical text. As a religious leader, Lin was inspired by a proselytizing zeal that reached into all religious communities and all levels of Chinese society. Simple texts and diagrams were prepared for the instruction of the uneducated. This point is of particular interest in view of the fact that the nontheistic character of Lin's religion was at odds with the popular theism. In recognition of this cultural gap, Lin salvaged mythological lore as an aid to instruction by translating it into cosmological abstractions.

The task undertaken by the author was made especially difficult by the fact that in order to argue her case for the syncretic process, she has had to introduce her readers to the antecedent Buddhist, Taoist, and Confucian systems of mind self-cultivation, as well as to provide a lengthy explication of Lin Chao-en's own system. In light of this fact, one may well understand why this book appears, as a whole, to be imperfectly organized. The same ground is often covered more than once, and information necessary to an understanding of the inner alchemy is scattered through the commentaries on Lin's method in a late chapter. The nonspecialist reader may also be perplexed, as the reviewer was, by the reference to Wang Che as founder of the Golden Elixir school (p. 39) and by another reference to "the Southern or Perfect Realization School" (p. 40). Wang Che is ordinarily associated with Perfect Realization, and the latter is taken to be a northern school in contradistinction to its less well-known southern counterpart. Berling's identifications are probably correct, but the reader should be enlightened on these points. One may wish that the author had been better served by her editors.

Berling has demonstrated the importance of religious syncretism as a subject requiring careful study in Chinese historical contexts, and she has succeeded in the nearly impossible task of translating the arcane language of inner alchemy into terms intelligible to the layman. Implicit in her work are suggestions for the direction of further research. Why, for example, should the Ming period stand as the "heyday of syncretism"? And how are we to understand the historical interaction between mythical and demythologized world-views in China?

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M. SANJDORJ. *Manchu Chinese Colonial Rule in Northern Mongolia*. Translated by URGUNGE ONON. Preface by OWEN LATTIMORE. New York: St. Martin's Press or C. Hurst, London. 1980. Pp. xvi, 118. \$22.50.

With the publication of M. Sanjdorj's study, we now have two works in English translation that offer the Mongols' own view of their modern history. *The History of the Mongolian Peoples' Republic* (1976), translated by William Brown and Urgunge Onon, is a massive history of twentieth-century Mongolia; Sanjdorj's book is a brief study of Chinese penetration into Mongolia during the Ch'ing dynasty. The two books put forward interpretations that often differ from those of the majority of both Chinese historians and Western scholars. It is useful for Chinese and Russian historians who do not read Mongolian to have access to these works, although they may not subscribe to the Mongol views.

Sanjdorj emphasizes Ch'ing China's economic exploitation of the Mongols. He shows that the Mongol herdsmen needed trade with the Chinese, while the Chinese were not dependent on commerce with the Mongols. The Mongols required cotton cloth, grain, tea, nails, matches, scissors, needles, and guns, and they coveted silk, jewels, and herbal medicines. They sold wool and hides in return for the necessities they received from the Chinese. Unfortunately for the Mongols, however, "the period when the livestock products were available and sold did not always coincide with the demand for necessities such as flour, grain, clothing" (p. 42). The Mongols were forced to buy on credit, a practice that placed them in the hands of unscrupulous Chinese moneylenders. The Ch'ing government "limited" the interest on loans to 3 percent a month, but the Chinese illegally charged even higher rates. Since the Mongols were often unable to repay the interest, they found themselves perpetually in debt. To add insult to injury, the Chinese merchants, according to Sanjdorj, "brought the cheapest and worst goods from China and sold them in Mongolia at prices normally paid" for higher-quality merchandise (p. 89). In short, the Chinese took advantage of and impoverished the Mongols and retarded the development of the Mongol economy.

Sanjdorj's description and analysis are shaped by the limitations of his sources and by current politics. His primary sources are the unique and inaccessible (at least to Western scholars) Central Archives of the Mongolian Peoples' Republic. Since Sanjdorj apparently does not read Chinese, he relies on Russian secondary accounts of Ch'ing China. Thus he tends to present the Chinese in the worst possible light. The current coolness between China and the Mongolian Peoples' Republic certainly adds to his unflattering portrait of the Chinese of earlier times.

Despite its biases, Sanjdorj's book is valuable because it is based on material that has not been consulted by other scholars. The effort entailed in discounting Sanjdorj's biases is well worth it, as the historical details he offers are extremely useful.

MORRIS ROSSABI

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DENIS TWITCHETT and JOHN K. FAIRBANK, general editors. *The Cambridge History of China*. Volume 11, part 2, *Late Ch'ing, 1800-1911*. Edited by JOHN K. FAIRBANK and KWANG-CHING LIU. New York: Cambridge University Press. 1980. Pp. xx, 754. \$69.00.

Like volume 10 (part 1 of *Late Ch'ing*), this is a book of major importance. Its more than 700 pages and ten major essays by twelve scholars cannot be reviewed critically in the conventional space allotment. As in volume 10, each essay is an interpretive overview of a key aspect of the period, incorporating the cumulative monographic research of a uniquely productive three decades since the Pacific War. The problem is more acute since the greatest weight of research on China published since 1947 has concentrated on this very period. Volumes 10 and 11 provide indispensable guides. The best review is probably the editors' preface, which points out that "'China' is in fact one of the largest generalities used in modern speech" (a truth that underlines the problem faced by each contributor) and stresses the importance, as for volume 10, of "the view from Peking" rather than the more common preoccupation with peripheral foreign activity. In the Fairbankian phrases of the preface, "Stimulus, in short, is where you find it, and stimulus without response is no stimulus at all . . . the chief source of inspiration for China's future was still her past."

Albert Feuerwerker provides in chapter 1, "Economic Trends in the Late Ch'ing, 1870-1911," a masterful survey of a field whose difficulty is belied by his careful research; his balanced and thoughtful account should long remain the standard. He concludes with the basic point that, although the economy had lost its capacity to grow, it was not "in mortal agony" and still supported most people above minimum subsistence; ideological and political collapse were not matched in the economy. Political and ideological revolution hence were easier, but the hope that this formula could also produce rapid economic growth "continues to haunt Peking." The essay is suitably cautious in handling statistical data but surprisingly not in its use of crop-yield figures, notoriously the most doubtful. Chapter 2 on "Late Ch'ing Foreign Relations, 1866-1905" by Immanuel Hsu has the most difficult assignment to treat in essay style since so much

factual information must be presented; this is done deftly in the volume's longest chapter, but there is relatively little scope for larger reflections, although the discussion of the Ili crisis (not surprisingly in view of Hsu's earlier work) gives a briefly revealing picture of the debate over coastal versus continental priorities.

With the factual story told, Yen-p'ing Hao and Erh-min Wang can be more reflective in "Changing Views of Western Relations, 1840-95" and can also return more consistently to the view from Peking in detailing the slow, almost reluctant increase in knowledge of the West and Westerners through a rich catalogue of revealing samples of the variety of reactions, judgments, and prescriptions by the literati. K. C. Liu and R. J. Smith then return, in "The Military Challenge: The Northwest and the Coast," to the great debate touched on by Hsu, although again mainly through a detailed review of events: the emergence of the post-Taiping armies, the successful action against the Muslim revolts, the efforts toward a modern navy, the Sino-French and Sino-Japanese wars, and Liu Ming-ch'uan in Taiwan. One might have wished, perhaps unreasonably, for a little more perspective on the big picture.

Hao Chang's chapter on "Intellectual Change and the Reform Movement, 1890-98" gives a startling illustration of Chinese reluctance to face new realities—the Kiangnan Arsenal's published translations of Western works sold some 13,000 copies from the mid-1860s to the mid-1890s, while in Japan, Fukuzawa Yukichi's *Conditions in the West* sold 250,000 copies almost immediately after its publication in 1866. Chang's treatment of the ultimate emergence of the reform movement and its new ferment is comprehensive and imaginative, including his well-balanced epilogue "Legacies of the Reform Era." Marius Jansen's "Japan and the Chinese Revolution of 1911," the volume's shortest chapter, gives a masterful sketch of the Japanese role in the emergence of modern China, not merely in terms of events but as an interpretive essay on changing mutual perceptions, influences, and interactions. Chuzo Ichiko then surveys "Political and Institutional Reform, 1901-11": education, the military, civil administration and constitutionalism, finance, and law. He concludes that reform was hamstrung by contradictions and by conflicts among self-serving groups, but it is surely a misleading partial truth to say that "the reforms led to the fall of the dynasty" (p. 415). Wellington Chan surveys "Government, Merchants, and Industry to 1911" in an essay that draws on his earlier work on the same subject but emphasizes a China-centered view of modern-style development in this period that captures the flavor and components of that discouraging story.

Michael Gasster's chapter on "The Republican

Revolutionary Movement" acknowledges, at least in its references, the massive contribution of Mary Wright's *China in Revolution* and implicitly underlines the appropriateness of the volume's dedication to her. Gasster's essay is a tour de force, a detailed narrative survey of a confusing cauldron of action and emotion through which he maintains an overall perspective and a judicious appraisal of exactly what "revolution" meant to the many contending groups and to China. "Currents of Social Change" by Marianne Bastid-Bruguier is suitably removed from the major events and surveys instead what was happening to each large social group as the developments earlier described began the transformation of the traditional system and led to the emergence of new groups, including a new "subproletariat," in "a society which had been slowly bereft of its soul and its spirit" (p. 602)—a fitting conclusion to the volume.

The scope of most of the chapters is inevitably such that the treatment must be once-over-lightly. But that is of course the volume's purpose, and it is achieved in a generally easy and literate narrative style, notwithstanding some unavoidable (and some avoidable) overlap. There is remarkable stylistic coherence, suggesting both a consistent editorial hand and a perhaps unconscious influence on the authors of the senior editor's long imprint on the writing of modern China's history; most of them were also his students. Literature and the arts are excluded by design from the treatment of the series; different readers will find different other matters slighted, but in general this is a superb guide, distilling the fruits of what may prove the greatest generation of scholarship on the last half century of the Ch'ing era. Extensive further guides to the literature are provided in separate bibliographic essays keyed to each chapter, followed by a 55-page bibliography that nevertheless confines itself only to works cited—a reminder of the volume's scope and also (since the bibliography is far from complete) of what it has left out. What it has done—by far the greater part of the job—is done gracefully, imaginatively, and well; it is in effect a tribute to the senior editor and to his long career as a shaper of the field.

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STEPHEN ENDICOTT. *James G. Endicott: Rebel Out of China*. Buffalo, N.Y.: University of Toronto Press. 1980. Pp. xii, 421. \$18.95.

Specialists and general readers alike who are interested in the history of Western Christian missionary activities in China and the role of the Christian church in a changing world are indebted to Ste-

phen Endicott for this thoughtful and highly readable biography of his father. James G. Endicott's career spanned a period in which new ideologies and social forces emerged to challenge the church's approach to social justice. His attempts to understand and, if possible, to reconcile the many conflicting ideological positions constitute the theme of this book.

The biography is divided into four parts. Part 1 deals with James Endicott's early years and his study and training for the ministry. Part 2 focuses on his work in Szechuan as a missionary of the United Church of Canada. In that capacity, he also served as a language specialist, professor of English and ethics at West China Union University, and a political adviser to Chiang Kai-shek's New Life Movement. Part 3 discusses his efforts to explain the Chinese Revolution and the events leading to his resignation from the ministry. The last section details his work in the Canadian Peace Congress and the World Council of Peace.

James Endicott's long career was distinguished by his indefatigable search for answers to questions concerning the nature and purposes of the missionary movement and the Christian church's response to war and revolution. His insistence on a rational and, at the same time, practical religion made him doubt the usefulness of certain outdated liturgical creeds and their relevance to countries such as China. Indeed, the attempt to impose Western church customs on the Chinese Christian community would only help to perpetuate the foreign character of Christianity and retard the growth of a truly indigenous church. In the 1920s, Chinese Christians were already agitating for an independent and self-supporting church. They called for the interpretation of Christianity in terms of China's own heritage and aspirations. The failure of most of the Western missionaries to sympathize with these demands and to hasten the devolution of power to the Chinese reinforced the charge of "cultural imperialism" leveled by many Chinese against the church.

The rising tide of Chinese nationalism posed a serious problem for the missionaries: what should be the church's response to the political and social upheavals? The author has skillfully analyzed James Endicott's inner conflicts over this issue and his final "political awakening"—the recognition that only the Chinese people themselves "acting mainly at the level of political change" (p. 132) could solve the problems of China. His experiences with the fascist Kuomintang Youth Corps and his involvement with the Communists strengthened his conviction of the need for a social revolution. He had finally crossed the "barrier between the viewpoint of an idealistic intellectual reformer and that of a committed revolutionary" (p. 206). In his view, the

church should not hesitate to take the side of the liberation of the people from their oppressors.

It was not surprising, therefore, that James Endicott spoke of the peace movement as an extension of his Christian experience in China. The common concern was the need to establish "a practical basis for universal brotherhood" (p. 306). But he soon discovered that for most of the Third World countries the immediate goal was the attainment of national and social justice through armed struggle, not the establishment of a universal brotherhood through peaceful coexistence and disarmament. It is not clear from the narrative if the victims of colonial wars had not interpreted the latter approach as a ploy of the superpowers to subvert their fight for national independence. It does bring to mind the position of the anti-Christians in China several decades earlier who had charged that the Western Christian missionaries employed the message of love and peace to weaken the Chinese will to resist Western imperialist exploitation and control!

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DONALD S. SUTTON. *Provincial Militarism and the Chinese Republic: The Yunnan Army, 1905-25*. (Michigan Studies on China.) Ann Arbor: University of Michigan Press. 1980. Pp. ix, 404. \$18.50.

Yunnan—geographically remote, economically straitened, and sparsely populated—has lain historically in China's political and cultural backwaters. For more than a decade after the 1911 revolution, however, this customarily insignificant province became a potent force in Chinese national affairs. In this prodigiously researched volume, Donald S. Sutton, a professor of history at Carnegie-Mellon University, explains the surprising efflorescence and subsequent diminution of the role of Yunnanese on the national stage.

Yunnan's prominence in China's political affairs was attributable to its army. Well funded with monies provided by Peking, and having superior systems of officer training and troop recruitment, the Yunnan army between 1909 and 1911 became the dominant military force in the southwest. Beginning in 1911, it also began flexing its political muscles. First, members of this new army assumed a leading role in Yunnan during the revolution against the Manchus. And then, as the revolution degenerated into political disorder and military fragmentation, the Yunnanese army moved into Szechuan and Kweichow, proclaiming lofty national goals. In fact, however, the army's commanders were motivated by the need for revenues to replace the subsidies formerly provided by the

Manchu central government. This "export militarism" by the Yunnanese army—by a provincial army with strongly provincial loyalties acting on the orders of a provincial government—obviously foreshadowed the subsequent emergence of warlordism. Sutton contends, however, that the unity and centralized command structure of the army immediately after the 1911 revolution distinguished it from warlordism *per se*.

Sutton also chronicles the historic role of the Yunnanese army and the charismatic figure of Ts'ai O during the famed "Third Revolution" of 1915-16, which led to the overthrow of Yuan Shih-k'ai's monarchical movement. He is even more interested, however, in the process by which the Yunnanese army, initially a relatively effective and coherent military organization, gradually decayed after 1916 and became simply a congeries of warlord forces. This process, he asserts, must be understood not simply as the result of evil men but of the confluence of disintegrative pressures such as the weakness of central rule, the fading of legitimizing goals, the burgeoning of armed units, and, above all, the paucity of money. As a consequence, the Yunnan army split into lesser forces, organized on personalistic rather than bureaucratic principles, dependent on local society, and progressively vulnerable to corruption. ("Goodwill," Sutton remarks, "was a depleting asset.")

During the latter years of its performance on the national stage, from 1916 to 1925, the Yunnan army became inextricably involved in the warlord politics of Kwangtung and in the revolutionary movement of Sun Yat-sen. These are events that have caused puzzlement to successive generations of graduate students (not to mention their mentors), and Sutton brings at least some preliminary order and sense to them.

Provincial Militarism is a notable contribution to the literature on the republican period, first because it narrates an important part of the history of south and southwest China during the first quarter of the twentieth century and, second, because it illuminates the organizational behavior of an army's officer corps during the descent into warlordism.

LLOYD E. EASTMAN
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NORBERT MEIENBERGER. *The Emergence of Constitutional Government in China (1905-1908): The Concept Sanctioned by the Empress Dowager Tz'u-hsi*. (Schweizer Asiastische Studien, Monographien, number 1.) Bern: Peter Lang. 1980. Pp. 115. 26.40 FR.

This is a study of a series of memorials and edicts marking the Ch'ing government's progress in shap-

ing a constitution. It begins with the edict of January 29, 1901, which launched the late Ch'ing reforms on the basis of what Norbert Meienberger calls "reform within tradition." It then jumps to the edict of July 16, 1905, which appointed a commission to study constitutional governments abroad. Other key documents analyzed are the constitutional commissioners' report (1906), the decision to adopt a constitution (1906), various edicts during 1906-07 reorganizing the administrative structure, the Nine Year Program of intended future administrative reforms (1908), and the Principles of the Constitution (1908), which succinctly defined the powers of monarch and parliament to be embodied in the future constitution. Meienberger stops here because at this point the court's conception of the coming order was clear. With the convening of provincial assemblies in 1909, the arena of debate widened; new ideas were introduced, and the timetable of reforms was speeded up. The dynasty fell in 1911 before its constitution was written.

Meienberger emphasizes two themes. First, he "underscores the achievements rather than the shortcomings" of the early reform period. The reforms, in his view, were substantial and "sincere," and the nine-year timetable was none too deliberate given the magnitude of the problems to be overcome. Second, he tries to show how the constitutional "alterations were designed to support and improve the *status quo* . . . [to] strengthen the Ch'ing dynasty." His analysis of each document is shrewd. He has thought hard about the bureaucratic implications of the intended reforms—which offices would gain power and which would lose—and he therefore illuminates not only the larger principles underlying the court's thinking but also the kind of order that would have been brought into being had the program come to fruition.

What makes this monograph interesting reading today—although Meienberger does not stress the point—is that many of the ideas adopted by the court continued to dominate Chinese constitution making throughout the twentieth century. For example, the idea of tutelage embodied in the Nine Year Program—gradually teaching the people how to operate in a constitutional system by a series of reforms from above—is still vital, as shown by the current introduction of direct election of county-level people's congresses as a step to eventual direct election of people's congresses at higher levels. The same can be said of the faith that a constitution links those "above" and "below" around a common goal and of the concept that the people's rights are granted by the state and can be suspended when necessary for reasons of state—both are alive in China today.

Meribeth E. Cameron treated the constitutional reforms from 1905 through the fall of the dynasty in

a chapter of *The Reform Movement in China, 1898-1912* (1931, reprinted 1963). She covered more material and analyzed these particular documents less closely than Meienberger does. E-tu Zen Sun's "The Chinese Constitutional Missions of 1905-1906" (*Journal of Modern History*, 24 [1952]: 215-68) studied the missions and their reports more thoroughly but did not bring the story up to 1908 in any detail. Meienberger's monograph is a useful addition to the literature on a reform that failed but left a heavy imprint on the thinking of later generations.

ANDREW J. NATHAN
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SOLOMON B. LEVINE and HISASHI KAWADA. *Human Resources in Japanese Industrial Development*. Princeton: Princeton University Press. 1980. Pp. xi, 332, \$20.00.

Among the spate of recent books on the Japanese employment system, management style, and labor relations, there is no other on the character of industrial training in Japan. Solomon B. Levine and the late Hisashi Kawada do not tell us all there is to know about how the Japanese trained their labor force, but in this excellent book they do provide substantial coverage of one major aspect of the history of Japanese industrial development not available elsewhere in a Western language.

Their focus is "primarily on institutions established or utilized in major large scale modern industries that have been leading sectors in Japan's achievement to become the second largest national economy of the world" (p. ix). They begin with an admirably succinct historical review in which statistical materials concerning Japan's human resources and economic development abound. The formal educational system is dealt with at length, and its strengths and weaknesses are highlighted with clarity. This background serves as essential introduction to the detailed consideration of employee training in ten modern industries: steel and shipbuilding; railways and telecommunications; banking, mining, and textiles; chemicals and electricity; and heavy machinery.

Although every case discussed is intrinsically interesting, and although each holds some surprises, it is the final chapter on problems and prospects that readers concerned less with Japan and more with the development of human resources in general will find the most rewarding. Even though the study is limited to a narrow range of industrially advanced sectors, the authors state categorically that "no single pattern emerges" (p. 283). On the comparative level, they find that although Japanese and American private enterprises have undergone

closely parallel development, "Japan's approach to the development of human resources has followed sharply different contours" (p. 283).

Having told us how the two differ, the authors move on to consider the implications of the Japanese case for the developing economies. It is an unexpected pleasure to find that they discern both positive and negative lessons for those who would consider emulating the Japanese. The Japanese have not done everything right, as they are so often misrepresented as having done, and the authors' balanced assessment of their performance is particularly welcome. They remind us forcibly that with regard to future problems and prospects, the strategies employed by the Japanese in developing human resources emerged over time as a response to a particular set of historical conditions and forces. The system, which is not immutable, will continue to develop in response to new pressures.

ROBERT J. SMITH
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WILLIAM FITCH MORTON. *Tanaka Giichi and Japan's China Policy*. (Studies of the East Asian Institute, Columbia University.) New York: St. Martin's Press. 1980. Pp. 329. \$27.50.

To a generation of Americans, Tanaka Giichi needs no special introduction. He stood accused as the author of the notorious "Memorial" to the emperor that surfaced in 1929 and outlined a grand plan of conquest beginning with China and extending eventually to India, Asia Minor, and even Europe. General Tanaka, prime minister of Japan from 1927 until shortly before his death in 1929, was thus widely branded as one of the sinister conspirators responsible for the aggressive Japanese policies that led to Pearl Harbor. The subject of Tanaka and his "Memorial" has been subjected to extensive scrutiny in Japan, but book-length treatment in English has been limited to half-century-old accounts with names like *Machiavelli of Japan*. All that is now changed with the appearance of this fine, even-handed study of Tanaka. William Fitch Morton has explored an impressive array of primary and secondary sources in Japanese, including Tanaka's papers, to produce a study that is likely to stand as the definitive political biography in English of one of the men most instrumental in shaping Japan's China policy in the 1930s.

As to the "Memorial," Morton concludes flatly that it is "spurious" (p. 109). Even the International Military Tribunal for the Far East, with all the resources at its command, could produce no more than weak and unsubstantiated statements concerning the existence of the document. At the same time, Morton notes that there are numerous Japa-

nese (and Chinese) scholars who, although doubting the existence of a formal memorial written by Tanaka (the internal evidence is overwhelming), still accept the view that some kind of a program of imperial expansion into China was established during Tanaka's tenure.

These remarks emphasizing the "Memorial" should not mislead the reader. Discussion of the "Memorial" is limited mainly to the appendix. The book analyzes the entire range of complex issues concerning the changing policies of Japan toward China in the crucial period when the nationalist tide of revolution reached its peak in the late 1920s. As Tanaka and the Seiyūkai political party confronted the rise of Chinese nationalism, they forcefully championed the notion that Manchuria and Mongolia were areas where Japanese influence must be increased. And although Tanaka's "positive policy" stopped short of plotting suppression of Chiang Kai-shek's revolution or creating a decisive attack on Chinese sovereignty south of the wall, Morton maintains that the "essentially civilian, economic approaches to China" that had characterized the diplomacy of Shidehara Kijūrō earlier in the decade gave way to "raw power" and a greater willingness to permit military officers on the scene in China to take matters into their own hands (p. 193). At the root of Japan's problems with China was the failure of Tanaka—and most of his contemporaries—to understand the revolution that was taking shape in China.

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AZUSA ŌYAMA. *Nihon gaikōshi kenkyū* [Studies in the Diplomatic History of Japan]. Tokyo: Ryōsho Fukyūkai. 1980. Pp. xii, 334. ¥2,500.

Publication of a collection of journal articles by a scholar is not commonly practiced in this country. It is rather frowned upon. But it is an accepted practice in Japan. Many Japanese scholars, especially those whose retirement is drawing near, take stock of all their published articles, select, and republish the most important ones in book form. Such a collection serves not only as a valedictory to the most creative phase of one's scholarly career, but also as unique service to foreign scholars whose access to Japanese-language journals is severely limited.

Ōyama Azusa, professor emeritus of Hiroshima University and an authority on the foreign relations of Meiji Japan, has brought out a collection of this sort. The twelve chapters of *Nihon gaikōshi kenkyū*, which chronologically cover the 1850s to the 1920s, were originally published in various scholarly jour-

nals. The anthology makes it no longer necessary for foreign scholars of modern Japan's foreign relations to seek, at enormous cost and energy, access to the specific journals in which the chapters previously appeared.

The twelve chapters are (1) "Japanese-American Treaties and the United States Consul General [Townsend Harris]"; (2) "The Ansei Treaties and the Opening of Open Cities and Open Ports"; (3) "The *Maria Luz* Incident and Its Legal Proceedings"; (4) "The Question of the Northern Territories during the Early Meiji Period"; (5) "The Question of Sovereignty over the Ryūkyūs and Sino-Japanese Disputes"; (6) "Treaty Revision and the Opening of Interiors"; (7) "The First Sino-Japanese War and the Surrender of Weihaiwei"; (8) "The Boxer Rebellion and the Amoi Incident"; (9) "The Russo-Japanese War and the Abolition of Military Administration"; (10) "The Foreign Policy of the Japanese Foreign Office during the Meiji Period"; (11) "The Japanese German Conflict and the Occupation of Chingtao"; and (12) "Chinese Merchants in Southeast Asia during the Taishō Period."

Ōyama wrote the articles while in the employ of the Office of Foreign Relations Papers (*gaikō bunsho shitsu*), the Japanese Ministry of Foreign Affairs, and subsequently teaching at Meiji and Hiroshima Universities. They are all based on invaluable original sources of information. The article that constitutes the second chapter of the book under review is the heart of his 1967 book, *Kaishi kaikō no kenkyū*.

RICHARD T. CHANG
University of Florida

PHILIP R. PICCIGALLO. *The Japanese on Trial: Allied War Crimes Operations in the East, 1945-1951*. Austin: University of Texas Press. 1979. Pp. xv, 292. \$15.00.

This is a solid, balanced, and readable overview of a complex and controversial subject: the Allied effort to determine the guilt or innocence of Japanese indicted as war criminals after World War II. As such, it serves as a persuasive counterargument to Richard Minear's brilliant and polemical *Victor's Justice: The Tokyo War Crimes Trial*, (1979). Where Minear's perspective and purposes are self-avowedly political, Philip R. Piccigallo's are decidedly not; where Minear centered his criticism on the IMTFC, the Tokyo trial of the "class A" suspects—the major Japanese military and civilian leaders—Piccigallo broadens his perspective to survey all the Allied war crimes tribunals, including those that dealt with the far more numerous "class B and C" suspects tried for conventional war crimes; where Minear criticized the assumptions of the Tokyo trial, Piccigallo accepts most (but not all) of these and sees the trial as a necessary and inevitable Al-

lied task; where the one faults the procedures of the Tokyo trial, the other draws attention to the essential fairness and impartiality of all the trials throughout the East.

In his early chapters, Piccigallo traces the process by which the United States came to play the dominant role in charting the scope and direction of the war crimes trials in the Far East. He describes the way in which the Nuremberg precedent—the broadening of the definition and application of the term "war crimes" to include the plotting and waging of "aggressive war"—became accepted as the basis for the Tokyo trial. In doing so, he underscores the process by which the Tokyo trial was shaped to contribute to American postwar objectives: the demilitarization and democratization of Japan and the realization of world peace based on international justice. If these purposes now seem either misguided or impractical, Piccigallo argues that they did not appear so at the time. Although Piccigallo concedes that the IMTFC may have erred in linking the Tokyo trial to any political objectives, he views it as a well-intentioned, if flawed, effort for which there was no reasonable alternative. On one major point, however, Piccigallo finds himself in essential agreement with both Richard Minear and Justice Radhabinod Pal, who, as Indian member of the IMTFC, provided the most ringing dissent to the majority opinion of the Tokyo trial. Piccigallo, Minear, and Pal all conclude that the accusation of a "conspiracy" to wage "aggressive war" ought to have been dropped from the charges against the defendants.

Piccigallo's major contribution to our understanding of the Allied war crimes operation is not his treatment of the Tokyo trial, but rather the attention he gives to the trials of nearly six thousand lesser Japanese for conventional war crimes. Piccigallo provides a comparative examination of each Allied nation's attitudes toward a treatment of Japanese war crimes suspects. Although discussion of the reasons for Japanese atrocities committed against Allied prisoners of war, civilians, and local populations lies outside Piccigallo's concerns, it is important to remember that conventional war crimes by Japanese military forces in Asian and Pacific combat theaters constitute an ineluctable fact in the history of Shōwa Japan. However persuaded one may be by Justice Pal's dismissal of a conspiracy to wage aggressive war, Piccigallo reminds us that the horrifying butcheries by Japanese troops from 1937 to 1945 were not "stray incidents," as Pal wished the IMTFC to believe.

In Piccigallo's opinion it was essential to the cause of peace and justice that Japanese suspected of committing these conventional crimes be brought to trial. (Minear is in essential agreement on this point.) Beyond this, Piccigallo believes that, by and

large, the more than two thousand tribunals that met to try these cases did a remarkably fair and effective job. Evidence was carefully sifted beforehand, the military courts were scrupulously impartial, the sentences meted out were often lenient, and those who were indicted were provided with competent, energetic, and responsible defense counsel. The great exception to these generally high standards of judicial procedure was the trial of General Yamashita Tomoyuki. Yet, Piccigallo persuasively argues, the Yamashita trial, tainted by haste, sensational pretrial publicity, and the passions of the recent war, was unique in these qualities, and he notes that Yamashita himself considered that he had been given a "fair trial."

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M. E. YAPP. *Strategies of British India: Britain, Iran, and Afghanistan, 1798-1850*. New York: Clarendon Press of Oxford University Press. 1980. Pp. viii, 682. \$118.00.

M. E. Yapp begins by admitting that readers may find something paradoxical in the general assumptions underlying his study of half a century's search for the best means of protecting India in the northwest. He does not take the Russian bogey seriously; defense against it he considers "neither the sole, nor even the principal determinant" of northwestern policies (p. 2). These he sees as the outcome of calculations in London and in Calcutta that often pulled in different directions. So far as the Indian government was concerned, its chief concern was always with "the internal enemy" (p. 584). Chronic fear of unrest and possible revolt inside the country did much to fix its rulers' thinking about external security and sometimes pushed them into ill-conceived adventures. This is convincing enough; far less so is the contention that Britain really cared little about India, at least until near the end of the period, and that it had no commercial aims in central Asia. According to Yapp, its aims there were, in some extraordinarily roundabout fashion, "Europe-centred" (p. 417). Marxism with its "forlorn, unreasonable attachment to economic motives" is summarily dismissed (p. 3). "If there was imperialism it was bureaucratic, not economic" (p. 15).

Whatever readers may make of Yapp's grand design, his book is very far from being six hundred pages of much ado about nothing. Not only does it incorporate an immense mass of research, obviously the labor of many years, but it is also full of challenging ideas and fresh appraisals. On the governmental side of the relationship between Britain and India it is illuminating about Whig-Tory discords,

divided responsibility between Foreign Office and Board of Control, and the fact that ministries were often too weak in parliament to be able to dictate to the company and its henchmen in Calcutta. Vital decisions about the northwest might be taken in India "because they were *not* taken in London" (p. 272). Chapter 9 gives an excellent description of the British occupation of Afghanistan during 1839-41, which was clearly intended to be permanent. Its failure and heavy costs are shown as reinforcing thoughts, already astir, of taking over Sind and the Punjab as consolation prizes. The final chapters are a rewarding study of how their annexation came about.

Very much of the work's strength, as well as its one-sidedness, lies in its recognition of the part played by individual motives and influences and the irrationalities they import into history. All the characters are brought vividly to life; comments on the successive governors general provide a regular portrait gallery. Even more attention is given to their agents, the political officers on the frontiers by whom, Yapp insists, "the great majority of the strategic discussions of the time were initiated" (p. 127). Some of these men were corrupt, the majority no worse than ambitious, eager for promotion and higher salaries; they were actuated in other words by those "economic motives" of which the British (though not the Russian) empire was so happily innocent. The effect was to "institutionalize expansion" (p. 183). Among them all, Sir John Malcolm stands out as the archetypal figure and a "skilful and merciless intriguer" (p. 53).

In all this there is much acute insight into the behavior of man as an official animal. The writing is always lively; despite its length the book never flags. Within its own, perhaps arbitrary, limits, it may deserve to be called a masterpiece.

V. G. KIERNAN
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BINA ROY BURMAN. *Religion and Politics in Tibet*. New Delhi: Vikas Publishing House; distributed by Biblio Distribution Center, Totowa, N.J. 1979. Pp. x, 180. \$14.00.

Bina Roy Burman's monograph is in essence a Ph.D. thesis. The author was for six years associated with the Education Department in Calcutta, has written several articles on Tibetan matters, and is presently involved in postdoctoral work on the internal structure of sects of Tibetan Buddhism. The emphasis in Burman's book is on politics rather than religion. The first three chapters, dealing with history, the past social and political structure, and the role of the monks and monasteries in Tibet, contain a useful compilation taken from the works of

the major Tibetologists, Sir Charles Bell, Hugh Richardson, Ram Rahul, W. D. Shakabpa, R. A. Stein, and others. Particularly valid is the account of the power of the leading monasteries vis-à-vis the thirteenth Dalai Lama (pp. 58–59), taken from Bell and Shakabpa. The concepts used by the author in describing the past social structure require, however, better definition and clarification. After speaking, for instance, of “small landed proprietors, rich farmers, merchants, small traders and headmen . . . tailors, carpenters and masons . . . smiths, tinkers and butchers” the author comes to the surprising conclusion that “apparently there was no middle class” (p. 25). The concepts of “feudalism,” “serfdom,” and other categories remain equally unclear.

The second part and chief emphasis of the book treats the Chinese policy of “democratic reforms” in Tibet. Burman attempts to balance limited interviews of Tibetan refugees with Chinese official and propaganda accounts, often taking the latter at face value. The result is a somewhat contradictory and confused picture of recent and present conditions that the author himself may want to revise when better investigation becomes possible.

FRANZ MICHAEL

George Washington University

T. J. S. GEORGE. *Revolt in Mindanao: The Rise of Islam in Philippine Politics*. New York: Oxford University Press. 1980. Pp. 294. \$21.00.

T. J. S. George, editor of *Asiaweek* in Hong Kong, grapples with the complex origins of Mindanao's recent and contemporary agonies in this readable volume. It is not a scholarly treatise. George's pages are unencumbered by footnotes, and his bibliography—consisting of only twenty-five uneven entries—is not of the weighty variety. His treatment of the Spanish and American legacies in the Philippines, moreover, is frequently cavalier and occasionally inaccurate. But these ostensible deficiencies do not undermine the book's value. George, after all, is neither a professional historian nor an academician. He is a practicing journalist. As such, he brings the reporting skills and special perceptions of his craft to an intricate subject. The results deserve serious consideration.

His endeavor to sort out the intertwined strands that produced religious strife and organized rebellion in Mindanao led George to stress three factors. First, diverging historical experiences under Spain, the United States, and the Philippine Republic intensified cultural differences between Islamic and Christian islanders. To this day, consequently, Muslims refer to the central administration in Manila as the “government of a . . . foreign people” (p. 79).

Second, Filipino national leaders—distracted by pressing political, economic, and social problems in the populous north—continued to implement assimilation policies in the southern regions that were unacceptable to the Islamic inhabitants of Mindanao and Sulu. The ill-conceived approach suggested “that the best way to integrate the Muslim was to make him less a Muslim, to win him away from the special laws and conventions that ordained his life” (p. 89). Third, an ascending wave of land-hungry settlers from Luzon and the Visayas descended on the underdeveloped south during the postwar decades. By the early 1960s “as many as 3,200 people a week were disembarking in Mindanao” (p. 115). The converging pressures created secessionist yearnings among Muslims, which erupted finally in full-scale rebellion.

George's reconstruction of the general and specific sources of the upheaval permits him to formulate a series of interpretations that adversaries of Ferdinand Marcos will not find particularly palatable. For one thing, he draws a fine distinction between the 1972 proclamation of martial law and the almost simultaneous outbreak of the abortive revolution. His position on the issue challenges those who see a cause-and-effect relationship between the juxtaposed events. For another, George grants primary credit for resolving the protracted crisis to President Marcos. Arguing that the most dangerous aspects of the situation had been defused by 1978, he praises the martial law leader for the patient diplomacy that terminated assistance from a radical segment of the Muslim world to the insular rebels. He also applauds the president's domestic policies of attraction, which “separated the leadership thrust of the insurrection from its mass base” (p. 242).

Whether, as George contends, the approaches of Ferdinand Marcos have transformed Philippine Muslims from a “persecuted” to a potentially “pampered” minority (p. 277) remains to be seen. In the meantime, he has provided a vivid and discerning account of recent developments in Mindanao and Sulu—a region most specialists in Philippine history have either slighted or ignored.

DAVID R. STURTEVANT
Muskingum College

UNITED STATES

KAREN ORDAHL KUPPERMAN. *Settling with the Indians: The Meeting of English and Indian Cultures in America, 1580–1640*. Totowa, N.J.: Rowman and Littlefield. 1980. Pp. x, 224. \$19.50.

Unlike H. C. Porter's recent narrative work on a similar theme (*The Inconstant Savage: England and the*

North American Indian, 1500-1660 [1979]), this is an argumentative book that makes a clear and generally persuasive case. The argument is that "neither savagery nor race was the important category for Englishmen looking at Indians. . . . English colonists assumed that Indians were racially similar to themselves and that savagery was a temporary condition which the Indians would quickly lose. The really important category was status" (p. 2). Indians, Karen Ordahl Kupperman argues, most resembled lower-class Englishmen; both were "perceived as possible subjects for exploitation" (p. 3). Racism set in only after some Englishmen late in this era reluctantly abandoned the ideal of assimilating the Indians and came to regard them as permanently different. That conclusion arose from American experience rather than from preconceived notions.

This book is the outgrowth of a 1978 Cambridge University dissertation. It is divided into topical chapters under two general headings: "English Writers Describe the Indians" and "English Culture Confronts Indian Culture." Along the way Kupperman finds that the English commonly respected Indian culture even when they disagreed with aspects of it. Accounts of Indian religious beliefs and practices were often expressed in terms comparable to those applied to "deviant" forms of Christianity, such as Roman Catholicism. Indian culture sometimes functioned better than European, as Englishmen occasionally recognized. It certainly appeared better adapted to the American environment than did the precarious first-generation colonies that perennially depended upon Indian-supplied food. Kupperman agrees with those who minimize the degree of European cultural superiority before 1640. Europeans' belief in witches, their supernatural explanations of disease, and their often clumsy technology were hardly more "modern" than the Indian equivalents.

Why then did Englishmen repeatedly refer to the Indians as savages? According to Kupperman, "savagage" apparently had no fixed meaning at the time and could be equated with "native"; it could be a neutral word and was often so employed.

Kupperman has delved deeply into the literature of which she writes, and her account is often ingenious and compelling. But as with many quasi-polemical books, understanding might better be served by a greater willingness to admit exceptions and qualifications. There is ample room for both here, given the diversity of her informants. The frequent stress on the colonists' vulnerability to Indian attack needs to be reconciled with the final emphasis on the "ultimate powerlessness of the Indians" (p. 188). And if Indians were exploited not because of race but because they fit all too well into the servile ranks of English society, as argued here, what shall we say about the simultaneous enslavement of Afri-

cans? Such cavils and questions aside, this is a provocative and important book.

ALLEN W. TRELEASE
University of North Carolina,
Greensboro

JESSE O. MCKEE and JON A. SCHLENKER. *The Choctaws: Cultural Evolution of a Native American Tribe*. Jackson: University Press of Mississippi. 1980. Pp. xx, 227. \$17.50.

This book adds little or nothing to what has already been published concerning Choctaw history and culture and suffers from an approach that is at best rigid chronicling and at worst an uncritical dependence upon quotations from the works of more serious scholars.

To start with its most serious failing: the book is grossly inadequate in research. Although the bibliography contains unspecific references to huge National Archives Record Groups (records of the Department of Interior, for example), the text and notes show only minimal use, all in the last chapter, of the rich material on the Choctaws in that repository. In fact, there is no noticeable use of primary material of any kind. For this age and this subject that is inexcusable in a book with any scholarly pretensions.

The book is offered as a study of tribal history and culture from prehistory to the present. But Jesse O. McKee and Jon A. Schlenker concede in the preface they would be satisfied if only the last chapter on the twentieth century added anything to already published material. The history section there is mostly a brief rehash of such broad issues as termination, and the concluding statistical profile of the present population of Mississippi Choctaws is hardly justification for a book. Scholars acquainted with Choctaw history in the nineteenth century need only know that Peter Pitchlynn rates a sentence in this volume and that Robert Ream is not mentioned at all to understand the inadequacies in other periods.

If it is not for scholars, maybe it is a well-written introductory volume for the general public, or perhaps a work of synthesis coming to insightful conclusions. It is neither. If one wants to read long quotations from the work of Gibson, Debo, DeRosier, and other historians of the Choctaws, it would be best to read their work in the original. This book hardly selects the gems for which those authors would like to be remembered, although almost all the analysis and style in the book come from such quotations. One of the longest quotes from Angie Debo is her colorless summary of the provisions of the Hopewell Treaty of 1786. A. M. Gibson is quoted while himself quoting a primary source in

an article from *Sooner Magazine*, while Arthur DeRosier is quoted at full-page length through a summary of the terms of the Treaty of Dancing Rabbit Creek (which is then included in its entirety in an appendix).

Books are too expensive to buy this one.

H. CRAIG MINER
Wichita State University

ARTHUR J. MEKEEL. *The Relation of the Quakers to the American Revolution*. Washington: University Press of America. 1979. Pp. vii, 368. \$12.00.

ARTHUR J. WORRALL. *Quakers in the Colonial Northeast*. Hanover, N.H.: University Press of New England. 1980. Pp. x, 238. \$12.50.

The huge growth in scope and detail of recent colonial history has included re-study of Quakers as well as Puritans. Humane ethics and narrative style were the strengths of the 1911 classic, *Quakers in the American Colonies* by Rufus Jones, Isaac Sharpless, and Amelia Mott Gummere (all of Haverford, none historians). But when the "Rowntree Series" of normative Quaker histories was brought forward for reprinting in 1961, William C. Braithwaite's *Beginnings and Second Period of Quakerism* (1912, 1921) were merely reissued with added footnotes by Henry Cadbury while three new volumes were planned in place of *Quakers in the American Colonies*. Edwin Bronner's on the middle colonies and Kenneth Carroll on southern colony Friends are still unpublished, but *Quakers in the Colonial Northeast* by Arthur J. Worrall, now of Colorado State University, is out.

The wealth of detail from local and Yearly Meeting records in all three books will never need recanting. Worrall carefully uses the document collections for each settlement (such as Pulsifer's and Langdon's on Plymouth) and also summarizes all the surviving scrupulously kept Quaker Monthly Meeting Minute-books: in seven appendixes taken from them he tabulates the disciplinary "Dealings" over morals and intermarriages, and the resulting "acknowledgements" of guilt or else "disownments." Quaker Meeting discipline was harshest in the older Meetings, notably after 1770. The vivid stories and autobiography in the older histories Worrall reduces to single sentences or pages, on Mary Dyer, the "Boston martyrs," the Flushing Remonstrance, and John Easton's mission to forestall King Philip's War. Worrall rarely portrays personalities. Worship and theology are shown mainly in Job Scott through one vivid fragment and a closing chapter miscalled "Retrospect and Prospect." Yet Worrall's pithy style, dropping half-sentences or wit as on distressing church taxes, comes into its own in showing Quaker political involvements, especially

in Rhode Island; in Friends' endless tussles elsewhere over oaths, militia, and taxes; and in Quaker slave-owning, slave trading, and their ending. Worrall's firm topical grouping of materials leads to some redundancy as periods and regions also subdivide his fields. In general, New Hampshire, and the largely Quaker Nantucket and Rhode Island showed generosity and evoked compromises by Friends, more than New York and Puritan Boston and Connecticut. Quaker schools and philanthropy are shown as arising late in the century.

Worrall's book, apart from the burying of a map, got better justice in publishing than Arthur J. Mekeel's *The Relation of the Quakers to the American Revolution*, with unjustified margins, thirty-five-odd typos, and notes buried at chapter ends. Mekeel's work was rounded out as a Fellow at Haverford from a project begun as a thesis in 1938. It adds lively readability to a data gathering as masterful as Worrall's. The "Virginia Exiles" and Quaker relief to besieged Boston are well detailed, again mainly in closing chapters. The main clashes and crises between the British Parliament, the crown, and the colonies, leading up to the Revolution, are made clear while showing the Quakers' relationships to laws, tariffs, and non-importation agreements. The intense inner political and religious rivalry within Pennsylvania that made the new constitution of 1776 a major revolution is shown in contrast to the continuation as "the new governments" of the old ruling groups in New England. Hence it was in Pennsylvania the Quakers clung most outspokenly to their posture as loyal, if at times nonviolent resisters to the crown, and Friends there were in turn much more brutally attacked as Loyalists than in the northern colonies. Like Worrall, Mekeel has worked intensively in English as well as American manuscripts. If, like Hermann Wellenreuther's *Glaube und Politik in Pennsylvania, 1681-1776*, he may give disproportionate weight to Dr. John Fothergill's letters to John Pemberton, Mekeel shows the importance of English Quaker agitation for colonial freedom, and the sense of trans-Atlantic unity of Friends who shared common citizenship and worship.

Mekeel's later chapters on war experiences, colony by colony, overlap Worrall's in content and style: repetition reinforces the issues—militia laws, Quaker exemptions, crises that in 1777-78 drove colonial legislatures to enact fines in lieu of service, war taxes and loyalty oaths with modern overtones. And always in the background the anguished question: when must pacifists reject two warring governments equally seriously, when must they transfer allegiance?

Once again, it is good that Mekeel's exhaustive detail and balanced detachment in these books can still be complemented by the rich and concise grasp of theology, ethics, schools, and personality develop-

ment in Jerry William Frost's *The Quaker Family in Colonial America* (1973) and the argumentative verve of Wellenreuther or Richard Bauman's *For the Reputation of Truths* (1971).

HUGH BARBOUR
Earlham College

ALICE HANSON JONES. *Wealth of a Nation To Be: The American Colonies on the Eve of the Revolution*. New York: Columbia University Press. 1980. Pp. xxxvi, 494. \$25.00.

This fine contribution was initiated in 1965 when Alice Hanson Jones first began to study probate records, tax lists, and other historical records on early American wealth holdings. Building from the dissertation, her publications appear to follow a pattern frequently used by many other scholars: mine the dissertation chapters for articles, extend and revise it into a scholarly monograph, and finally, complete a second book to popularize the findings. Her articles on wealth estimates for each of the colonial regions, which are listed in the bibliography, significantly advanced our understanding of the material standards of living in the late colonial period. And only recently, she completed her three-volume study *American Colonial Wealth: Documents and Methods* (1977; rev. ed. 1978).

It must be emphasized, however, especially for those familiar with her earlier works, that *Wealth of a Nation To Be* is far more than a mere popularization of past efforts. Indeed, this is a masterpiece of historical investigation, synthesis, and analysis. The broad sweep of wealth and income comparisons to other countries in the late colonial period and to developing and developed countries today compares favorably to the scope and design of studies by Nobel laureate Simon Kuznets. Ample attention is paid to estimates made by others, and Jones is particularly successful in reaching a synthesis of findings and in placing earlier works in new perspective. Her conclusions on growth rates and wealth distribution are not entirely free of conjecture, nor are they beyond dispute. Indeed, the absence of hard information on the wealth of those not leaving probate records forms a void that demands scholarly judgment and ensures debate. Nevertheless, her numerous calculations, tables, and figures are by far the most thorough, carefully reasoned, and thoughtfully explained estimates available.

Although the findings are fascinating, the book is not enjoyable reading, and I wish it had been restructured. I recommend that the summary and conclusion chapter be read before taking up the sequence from the beginning. The early chapters are painfully dull and tedious, filled with every assump-

tion, statistical qualification, and data limitation in need of explanation. No special skills are required to understand these necessary but taxing explanations. Without seeing the larger issues and ultimate findings, however, the early tedium may tempt many to lay the book down. By the third chapter the narrative and findings begin to flow—on aggregate and per capita wealth holdings in the colonies, on the form and structure of those holdings, and on the distribution of wealth. Vexing and fascinating historical issues are addressed, and comparisons are made to other times and places. Despite these caveats, the careful reader will be amply rewarded, overall, and no scholar of American history can afford to be unfamiliar with this exceptionally valuable study.

GARY M. WALTON
University of Miami

JAMES H. HUTSON. *John Adams and the Diplomacy of the American Revolution*. Lexington: University Press of Kentucky. 1980. Pp. vii, 199. \$13.00.

This provocative monograph seeks to keep the focus on John Adams in an otherwise conventional account of the course of diplomacy during the Revolutionary years. It depicts Adams as the epitome of that Revolutionary mentality infused by suspicions of "malevolent conspiracy." James H. Hutson sees Adams as a powerful intellect "in full control" of diplomatic issues from 1774 to 1776. Thereafter he is described as the sport of passions, which threaten to wreck both Adams and the affairs in which he meddled, although he does somehow manage to extricate himself with honor.

Save perhaps for its lopsided presentation, this portrait has little novelty. We read the standard account of the Lee-Deane feud and of Adams's running battle with Vergennes. The latter misses the telling point Adams scored in his argument with Vergennes in the summer of 1781 over the Austro-Russian mediation. Adams's strictures on the need to deal with Congress rather than the separate states in all diplomatic overtures was indeed a crucial point for America in the winning of independence. Annoyed though Vergennes may well have been by Adams's pedantic posturing, the Frenchman did incorporate in his reply to the mediators the gist of the argument Adams had pressed. For a person allegedly afflicted with a paranoid disorder, Adams seemed remarkably shrewd and balanced in his comprehension of the Dutch situation and his triumphant negotiations at The Hague.

If the psychological explanations offered by Hutson for Adams's behavior are at most suggestive, his analysis of Adams's diplomatic ideas is even less persuasive. Hutson disagrees with Francis Wharton

and Felix Gilbert in their categorization of American diplomacy in the Revolutionary era as "progressive," with its repudiation of traditional diplomacy and power politics. Instead, he insists that Adams was a convert to the balance-of-power concept that dominated American diplomacy. Hutson rejects Gilbert's description of the diplomatic ideas of both the *philosophes* and the Americans as combining idealism and internationalism, including freedom of trade. Not so, argues Hutson, who finds that Adams's "Model Treaty," superficially analyzed herein, favored commercial reciprocity rather than freedom from restraint. In short, except for the Dutch interlude, Hutson would have us believe that Adams proceeded in a straight line from the Declaration of Independence onward, a line he describes as "customary, European, and conservative" (p. 155).

This view that Adams and his associates favored a continuation of mercantilist policies distorts the facts. David Hartley, whose ideas reflected the viewpoint of the American peace commissioners in Paris much more closely than they did the British government's, reminded Charles James Fox in 1783 that "as surely as the Rights of Mankind have been established by the American War, so surely will all the acts of Navigation of the world perish and be buried among occult Qualities" (Hartley MSS, W. L. Clements Library). John Jay, in turn, would have liberated trade between America and Ireland from prewar restrictions. As for Adams, he sought to incorporate in the trade treaty with Britain a stipulation for freedom of navigation and was prepared to enter into a mutual guarantee that "no forts shall be built or garrisons maintained upon any of the frontiers in America, nor upon any of the land boundaries," a path-breaking notion not completely implemented until the later Treaty of Washington of 1871. Franklin, though not the subject of Hutson's study, anticipated some of the advanced ideas about protecting noncombatants, ideas embodied in the treaty of commerce with Prussia, of which John Adams was a co-signer.

In sum, the author fails to comprehend the central points of the diplomacy that Adams and his colleagues espoused, including the right of a sovereign people to choose their own form of government, moves toward free trade, and humanitarian innovations, innovative notions that would liberate American diplomacy from traditional European shackles.

RICHARD B. MORRIS
Columbia University

ROBERT A. BECKER. *Revolution, Reform, and the Politics of American Taxation, 1763-1783*. Baton Rouge: Louisiana State University Press. 1980. Pp. xi, 323. \$25.00.

Robert A. Becker has produced a thorough study of how the various colonies and states raised general public revenues in the Revolutionary era. Since the book is narrow in scope—Becker purposely excludes systematic discussion of local and religious taxation as well as national finance—it will be of interest primarily to students of provincial and state government. Historians concerned with the distribution of wealth in the Revolutionary era may also find Becker's findings useful.

The first half of the volume covers the period before the war in chapters entitled "New England," "The Middle Colonies," and "The South"; the second half does the same for the war years. All chapters discuss the same kinds of specific subjects—the forms of taxation, the mechanisms of tax collection, the distribution of the tax burden among the citizenry, and political disputes over tax legislation. Since Becker finds generalizations about regions difficult, he has, for the most part, written about individual colonies and emphasized uniqueness. Becker does, however, discern one major trend apparent in all of America, the increasing concern for what he labels "reform." By reform Becker does not mean progressive or graduated taxation, but taxation based on the assumption that "all should pay an equal proportion of their wealth or income in taxes" (p. x). Inasmuch as legislators continued to rely on poll taxes or land taxes in which both improved and unimproved land were evaluated the same, they resisted reform. Taxation based on the market value of property and the "faculties" or abilities of individuals to produce income was consistent with the idea of reform. The central theme of *Revolution, Reform, and the Politics of American Taxation, 1763-1783* is that the Revolution stimulated efforts, many of which were successful, to alter the system of raising public revenues in the direction of reform.

In general Becker argues his case well. He is careful not to exaggerate either the frequency or the success of reform efforts. He acknowledges the role of both ideology and self-interest in shaping the politics of taxation, and his narrative of events keeps the two in balance. Becker may be too quick to assume that reform had the effect intended by the legislators, but his concern is the politics of taxation more than the economic effect of tax legislation. Since most historians of states during the Revolution, this reviewer included, have all but ignored the subject Becker has examined so thoroughly, we should be grateful for his effort.

JERE R. DANIELL
Dartmouth College

RODNEY ATWOOD. *The Hessians: Mercenaries from Hessen-Kassel in the American Revolution*. New York: Cambridge University Press. 1980. Pp. xi, 292. \$28.50.

The German auxiliaries who loomed large in the folklore of the American Revolution have never been the subject of full and satisfactory studies in English, although good work has been done in German in the past twenty-five years on the contingents from Brunswick and Hessen-Kassel. Now Rodney Atwood's book brings together material from a great range of British and German sources, including seven German manuscript repositories. The result is a balanced and well-rounded study of the Hessen-Kassel force which contributes to our knowledge of the War of Independence and provides an interesting picture of the military organization of a secondary German state. There are many incidental points of interest in this well-written book that will appeal to the student of war and of military society and that shed interesting light on America.

Three chapters out of twelve deal with operations in America, including one on the battle of Trenton. The Hessians emerge as brave, loyal, and professional. Atwood's evidence suggests, however, that their command was unimaginative, and that, because they always served as auxiliaries, few of the senior officers developed the higher qualities of leadership. Their tactics were successful. Spear-headed by their green-clad *Jäger*, possibly the best troops on either side in the war, they brought with them from Europe considerable skill in the art of flexible attack, covering their line with riflemen and skirmishers. Their line remained close knit and consequently slower moving than the British, but there is little doubt that when they returned to Europe they were tactically the most competent and progressive troops on the continent.

Plundering, desertion, and relations with the British are each given a chapter. But the most interesting parts of the book are the four chapters on the home base in Hessen-Kassel, its society, and military organization. The landgraf's army was probably then the best in Germany, and in proportion to population it was the largest. Like Prussia, Hessen-Kassel was organized to support its army, with recruiting cantons, registration of all fit males between sixteen and thirty, and exemption of the classes and occupations that provided the economic infrastructure. The army itself was used in mercantilist fashion as the state's principal export, and it showed a profit. Of the six German states that provided contingents for the American war, Hessen-Kassel was the only one that was financially stable, and Landgraf Friedrich II, an enlightened and paternalistic reformer, handed on a full treasury to his successor.

PIERS MACKESY
Pembroke College,
Oxford

CARL E. PRINCE, editor. *The Papers of William Livingston*. Volume 1, June 1774–June 1777. Trenton: New Jersey Historical Commission. 1979. Pp. xxvi, 434. \$30.00.

William Livingston deserves to be, if the word exists, an editée. He was governor of New Jersey when the state was in the vortex of the War of Independence, and he continued in office until his death in 1790. This volume reveals his personality, as always in a man's papers, only in flashes. The rhetoric of his public pronouncements was orotund and conventional, and the one example given of his satire scarcely explains why he was celebrated as a polemicist. He showed a lawyer's caution, bordering at times on the finicky, in exercising as governor an authority that the new constitution did not fully define. The problems of improvising a war weighed heavily upon him. He made no bones about them, and particularly about the quality of the soldiers: they aroused his contempt. He described the Pennsylvania associators who came to the aid of his state as a "discipline-hating good-living-loving too eternal-fam'd damn'd coxcomatical crew" (p. 129). His own militiamen were no better. He was ready to call them out to face the British invasion, he wrote Washington; but God knew whether they would obey the order, and he himself knew how little use they would be if they did (p. 175). As the enemy advanced, the militia disintegrated and the government with it. After mid-December Livingston's papers have a gap of more than a month, during which his whereabouts were and still are unknown; depositions subsequently laid before him show the chaos of those weeks. Drama, though well hidden in the mass of the papers, is there for the seeking.

The crucial question for the editors, as in any selective edition, is what to select. Until they produce their promised microfilm of the entire corpus, no one can judge how wisely they have chosen, for they do not spell out the criteria that guided them. "Routine communications from Livingston," they say, are summarized or omitted (p. xiv); but what is routine? Not a state paper that deals with moving a courthouse or with the resignation of a lieutenant colonel, not the numerous individual commissions that the governor signed; these are printed in full. So is material to which he was exposed, such as the depositions late in the volume; vivid as some of these are, they are unduly repetitious. A small group of documents is relegated to an appendix, for a reason that may be good but is not explained. Canons of selection are impossible to define precisely but can be laid out in general terms.

The editorial apparatus, impeccable as it is for the most part, could be improved in a few details. Missing words that may reasonably be conjectured

are supplied in brackets (pp. xix–xx), yet the first document contains two bracketed adjectives, “calculable” and “destruct,” which are odd conjectures; when missing words cannot be guessed, footnotes often say how many there were, which is divination. The excellent biographical directory is insufficiently advertised; a few references to it in the initial annotation would be helpful. Such small criticisms may appear carping. But this first volume in a series of five (the second has just appeared) is so well done that even minor suggestions for improvement are in order. Editorial technique, like literary style, is not static; it needs constant refining, or it would not be the challenge that it is.

WILLIAM B. WILLCOX
Papers of Benjamin Franklin

RICHARD K. SHOWMAN, editor. *The Papers of General Nathanael Greene*. Volume 2, 1 January 1777–16 October 1778. Chapel Hill: University of North Carolina Press, for the Rhode Island Historical Society. 1980. Pp. xxxix, 606. \$22.00.

The second volume of Nathanael Greene's papers covers the most active part of his military career in the northern theater of operations, spanning the period from New Year's Day of 1777 to mid-October 1778. During the whole time, he was with the army, except for a week he spent with his family in Rhode Island. He fought in the battles of Princeton, Brandywine, Germantown, Monmouth, and Newport, accepted the appointment of quartermaster general in March 1778, and kept claiming that he had the right to command troops while holding down this administrative post.

Scholars will find the second volume more rewarding than the first for several reasons. Greene played a critical role in more crucial battles, especially at Brandywine where he saved part of the American army. More of his papers survived for this period than that covered in the first volume. And his duties as quartermaster general produced a greater volume of correspondence. The result is 530 carefully selected documents, but because of the mass of correspondence 170 of them have been calendared and abstracted only.

In the current controversy raging in editorial circles, the editors have followed the practices established by the late Julian Boyd, Leonard Labaree, and others. The manuscripts are treated in such a way as to make them intelligible to the modern-day reader while retaining as much as possible the essential form and spirit of the writer. These practices reflect a compromise to which certain scholars will take exception.

The only criticism this reviewer has is to the

judgmental character of certain footnotes that sometimes contain gratuitous remarks. A few examples might suffice: “Howe's intentions were as confusing to his staff (and perhaps to himself)” (p. 97); “Anyone with a claim to military expertise could only assume that Howe's next move . . .” (p. 107); and “A stronger assumption by Congress might have gone like this” (p. 11).

This minor criticism aside, the work maintains the high level of excellence established by the first volume. The papers as a whole will provide scholars with an opportunity to re-evaluate Greene and his contribution to the Revolution.

GEORGE ATHAN BILLIAS
Clark University

DREW R. MCCOY. *The Elusive Republic: Political Economy in Jeffersonian America*. Chapel Hill: University of North Carolina Press, for the Institute of Early American History and Culture, Williamsburg, Va. 1980. Pp. ix, 268. \$21.50.

Despite the growing body of literature on the ideology of republicanism in the late colonial and early national periods, the central dilemma of the effort to create a republican system of political economy has, until now, remained largely unexplored. Classical theory, as modified in the seventeenth and eighteenth centuries, held that republics could survive only if the people were virtuous, which was possible only if wealth were minimal and fairly evenly distributed; and yet both theory and experience taught that inequalities of wealth would arise and progressively increase in any free society. How the Jeffersonian Republicans tried to solve this insoluble problem is the subject of this excellent study by Drew R. McCoy.

The Jeffersonians conceived of economic activity in terms of the stages-of-development model—from hunting to herding to agriculture to commerce—worked out largely by the Scottish Enlightenment. At the earliest stages, society was nasty and brutish; at the latest, men succumbed to a love of luxury that was fatal to republics. The trick, then, was to freeze American development at an intermediate stage. Madison and Jefferson sought to achieve this, as McCoy puts it, by instituting a system of political economy that would channel development across space rather than through time. That is, they would avoid the corruption attendant upon the “higher stages” of capitalistic manufacturing and extensive commerce by a policy of territorial expansion, which would enable the United States to continue to be mainly a nation of farmers, and by a policy of free trade, which would provide a market for expanding agricultural surpluses and enable Ameri-

cans to buy their necessary manufactured goods abroad. The weakness in this otherwise excellent scheme was its lack of relation to reality: it required an aggressive foreign policy and continuous involvement in European affairs, and thus "the national independence and isolated self-sufficiency boasted of by the Jeffersonians were illusory" (p. 205).

The book is not without shortcomings. Hamilton's system of political economy is treated in cursory fashion, with the results that we are left (1) with the mistaken impression that his system was a Mandevillian rejection of republican virtue rather than Hamilton's own alternate form of establishing public virtue, and (2) with an inadequate understanding of how Hamilton's system ultimately prevailed despite being defeated politically. Related to these problems are others, including neglect of the crucially important transformation of American law, of the attitudes of Northern Republican businessmen on the make, and of the more extreme Virginia Republicans such as John Taylor and John Randolph. But it is unfair to criticize an author for a book he did not write. Within the limits McCoy set for himself, he has handled the subject with remarkable subtlety and skill.

FORREST MCDONALD
University of Alabama

DAVID P. SZATMARY. *Shays' Rebellion: The Making of an Agrarian Insurrection*. Amherst: University of Massachusetts Press. 1980. Pp. xiv, 184. \$14.00.

Students of the American Revolution who have been waiting for something on Shays's Rebellion that draws from the rich hoard of material in the Massachusetts Archives and Massachusetts Historical Society, as well as from the growing body of scholarly writings on revolutionary Massachusetts, will open this book with interest. David P. Szatmary argues that the rebellion grew out of a clash between a traditional, family-centered, communal agrarianism and an expansive, individualistic, market-oriented commercialism. His analysis of these two cultures locates the source of the trouble in the actions of coastal merchants who, because they were unable to establish new commercial circuits after the war, were forced back upon their old, prewar reliance on British merchants. Their efforts to re-establish their credit in that quarter led them to put pressure on inland retailers to collect debts from their country customers, thereby exacerbating "an already existing antagonism between merchants and yeomen" (p. 20). The difficulties of the yeomen were compounded by the state government, wedded to the interests of the commercial creditor, which embarked on a stringent program of tax-

ation. And the rebellion erupted when the agrarians broke under these dual pressures.

Using Hobsbawm's categories, Szatmary argues that the yeomanry first supported "reformist measures" (p. 37) such as proposals for paper money and tender laws. When these were rejected by the legislature, they threatened violence; and when nothing happened then, they acted on the threat. But the violence directed toward closing the courts remained reformist in tendency at first and only became revolutionary when the government seized and imprisoned some of the Shaysite leadership in November 1786. Szatmary feels that although the other states contained seeds of similar rebellions, they avoided the fate of Massachusetts because of decisive repression (Connecticut and New Hampshire), or because the government wisely temporized, or, in one place (Rhode Island), because it sided with the agrarians. But the leaders of the commercial culture throughout the nation felt sufficiently threatened by developments in Massachusetts to unite behind a revision of the Articles of Confederation that resulted in the Constitution. Szatmary concludes by arguing that some provisions of the Constitution—those forbidding the states to emit bills of credit or pass tender laws and guaranteeing a republican form of government—stem directly from the rebellion and that opposition to ratification came principally from the agrarian sectors.

Though Szatmary's account presents the image of a moving body (the expansive, commercial culture) colliding with a stationary one (traditional agrarianism), an encounter he describes as "almost inevitable" (p. 37), he obviously holds the elite members of the commercial culture responsible. They are portrayed as pushing the farmers to the wall in their pursuit of self-interest, then stiffening into inflexibility when they met resistance. "They could have accepted proposals for paper money and tender laws, solving the immediate problem by a few legislative moves. They could have postponed the crisis by promising to consider rural suggestions after the new elections" (p. 76). Thus the author invites us to infer that they provoked a rebellion that they might as easily have avoided. He stops short of saying that they did this with deliberate intent to produce the strengthening of the national government that ensued, but in most respects his account of the rebellion places him within the populist tradition of American historiography. Adherents of this view will probably like his book. On the other hand, those who balk at interpreting debtor relief and debt repudiation as reform, those who wonder what kind of subsistence farmers these are who first go deeply into debt and then want to pay in a depreciating paper currency, and those who want a more sophisticated explanation of why Massachu-

setts erupted into civil war alone of all the states, will have to wait for some future study of the rebellion.

RICHARD BUEL, JR.
Wesleyan University

MICHAEL STEPHEN HINDUS. *Prison and Plantation: Crime, Justice, and Authority in Massachusetts and South Carolina, 1767-1878*. (Studies in Legal History.) Chapel Hill: University of North Carolina Press. 1980. Pp. xxviii, 285. \$20.00.

Two societies—separated by less than a thousand miles, colonized from England at about the same time, sharing the same legal heritage and a common experience of revolution—developed drastically different systems of justice. Nevertheless, the criminal processes of the two states served a similar end—the control of those who posed the greatest threat to the prevailing social and economic system. These very different approaches to the same objective constitute the central theme of Michael Stephen Hindus's excellent book.

Prison and Plantation charts the evolution of the criminal justice systems of Massachusetts and South Carolina within the context of the social and legal history of the two states. The book compares the various elements of the criminal justice system—the criminal law, police, courts, criminal procedure, the demographic composition of offenders, penal practices, and reform initiatives in each state. The legal process in Massachusetts was formal, while that in rural South Carolina differed markedly. Dueling was not an uncommon means of resolving disputes in the latter jurisdiction during the first half of the nineteenth century.

Those forms of deviance that preoccupied authorities also differed significantly across the two societies. In Massachusetts drunkenness, vice, and theft were antithetical to the maintenance of a diligent, disciplined work force. The foreign born, who constituted 21 percent of the Massachusetts population in 1860, were viewed as particularly threatening to public order. In South Carolina, offenses against property and authority by blacks (59 percent of the population in 1860) posed the greatest threats to a slave-based agricultural society. Assault, the offense most typically committed by South Carolina whites, was not regarded as a significant social problem in a society where a strong tradition of personal honor and sustained personal contact among members of a dominant racial minority rendered interpersonal violence more likely.

The prison and the plantation stood as the dominant institutions of penal policy in the two societies. Indeed, it was not until Reconstruction that South Carolina built a penitentiary. It is illustrative of the

book's general theme that, in terms both of the racial and ethnic composition of inmates and of the nature of prison work and routine, the prison finally constructed in South Carolina appeared much like a plantation, while Massachusetts State Prison retained many of the characteristics of a nineteenth-century factory.

Prison and Plantation provides an excellent model for future comparative research. Although the generation of hypotheses for future testing is not one of the book's stated goals, the author does at one point suggest that "only when property is threatened does a society willingly allocate resources to a penal system," a disputable but testable proposition. The book is generously leavened with illustrative statistics gleaned from the fragmentary records of each state. Hindus is sensitive to the forbidding impediments to the interpretation and comparison of historical statistics on crime and justice. He makes no untoward assumptions and promises no more than he delivers.

The book is not without shortcomings. In particular, the author could have keyed his discussion more explicitly to the work of Tilly, Rusche and Kirchheimer, Thompson, and other recent theorists of disorder and social control, not to mention Marx and Durkheim. But the work of theory building can be left to others, and Hindus's work stands well on its own. It is an important contribution to the growing literature on comparative historical studies of crime and criminal policy and will appeal to a diverse readership.

P. N. GRABOSKY
Office of Crime Statistics,
South Australia

ERNEST MCPHERSON LANDER, JR. *Reluctant Imperialists: Calhoun, The South Carolinians, and the Mexican War*. Baton Rouge: Louisiana State University Press. 1980. Pp. xiv, 189. \$13.95.

Ernest McPherson Lander, Jr., professor of history at Clemson and a dean of South Carolina historiography, adds another paradox to the paradoxical history of the state. *Reluctant Imperialists*, an account of South Carolina's participation in and attitudes toward the Mexican War, illuminates an unexpected reluctance. Antebellum South Carolina is supposed to have been leader of Southern extremism. Yet Landers shows that in the late 1840s, Calhounites were exceedingly reluctant to acquire extensive territories to their south. Led by John Calhoun, supposedly king of ultraism, these "extremists" waxed extreme only on the need to cut off the war before America consumed too rich a feast of tropical land.

Why such reluctance? Lander's book is too short,

and his other story too omnipresent, to provide a detailed answer. The author feels compelled to give over too many of his too few pages to the best account we have of how the ill-fated volunteer Palmetto regiment fared on Mexican battlefields. That story, while interesting, does not bear the wider analytical importance of the reluctance conundrum.

Not that Lander fails to provide answers to his own best question. He argues that Calhoun's maneuvers for a better political position, and Calhounites' fear that the Wilmot Proviso would be applied to all acquisitions, were pivotal reasons for unexpected anti-imperialism. These explanations, while essential, leave out something. That something is South Carolina's mysteriously eccentric character as a culture.

South Carolina reluctance to acquire more tropical land, after all, continued after Calhoun was dead and the Wilmot Proviso was irrelevant. In the late 1850s, Louisiana, not South Carolina, was in the forefront of Caribbean expansionism and constantly blasted South Carolina, of all places, for being laggard in the Southern cause. Yet South Carolina was in the forefront of breaking up the Union in 1860, ostensibly because Lincoln might stop Southern expansionism! Why such extremism for expansionism after such reluctance to expand?

A brief review is an even worse place for answering such questions than is Lander's brief book. One must instead thank him for pressing on our attention such large questions about a state history he has long helped to make so important to American national historians.

WILLIAM W. FREEHLING
Johns Hopkins University

THELMA JENNINGS. *The Nashville Convention: Southern Movement for Unity, 1848-1851*. Memphis: Memphis State University Press. 1980. Pp. vii, 309. \$16.95.

This well-researched volume is the first book-length study of the Southern efforts at unity in the sectional crisis of 1848-50 that centered around the Nashville convention. Called by the state of Mississippi to discuss various Southern grievances concerning abolitionist attacks on slavery, the Nashville meeting was viewed by Southern extremists as an opportunity to bring the slaveholding states together in a unified movement against Northern aggression. One hundred seventy-five delegates from nine states participated in the first session of the convention held during the first two weeks of June 1850. To the dismay of Southern extremists the work of the first session was essentially moderate in tone. After much discussion and debate, the delegates adopted twenty-eight resolutions and an address outlining Southern demands for continued

support of the Union. The convention then adjourned until November to await further actions of the Congress.

The second session of the convention was anti-climatic. Although the fifty-nine delegates who attended this session were more radical than those who participated in the June meeting, the work of the Congress in passing the Compromise of 1850 had muted Southern interest in extremism. Delegates to the second session adopted resolutions reaffirming the right of secession, denouncing the Compromise of 1850, and recommending a Southern congress to consider further action. Most Southerners were willing to give the compromise proposals a chance, however, so little of a tangible nature was achieved by the second session of the convention.

Most historians have treated the work of the Nashville convention as of little consequence. Thelma Jennings argues convincingly that such was not the case. Although she concedes that the convention failed its primary purpose of uniting the South against the North, Jennings contends that the convention brought national attention to the South's grievances and helped secure passage of the compromise measures of 1850. She also points out that the convention demonstrated that the majority of Southerners were still loyal to the Union in 1850 and willing to compromise in order to preserve national unity.

This is a useful volume. Although it contains little that is startling or new, it fills a gap in our knowledge of events in the pre-Civil War era. It also helps in an understanding of the events of 1860-61, for, as Jennings correctly observes, Southern nationalists learned much from the 1848-50 efforts at Southern unity. The difficulty in securing support of all the slaveholding states convinced extremists that secession could be attained only through separate state action rather than through cooperative action such as that attempted at Nashville.

RALPH A. WOOSTER
Lamar University

MARGARET HOPE BACON. *Valiant Friend: The Life of Lucretia Mott*. New York: Walker. 1980. Pp. x, 265. \$14.95.

Some contemporaries viewed Lucretia Mott, touchstone for the collective conscience of nineteenth-century abolitionists, women's rights advocates, and peace reformers, as a saint. Others saw her as an infidel, "a worshipper after the way called heresy," as she once termed it. Yet both saints and infidels have human aspects. In this, the first full-length Mott biography since Otelia Cromwell's *Lucretia Mott*

(1958), Margaret Hope Bacon has succeeded, with clarity and simplicity worthy of Mott herself, in portraying Mott's human as well as her spiritual qualities. Relying heavily on Mott's own writings, Bacon balances the "warm and loving woman of great poise" with the "very human person with a quick temper, a sharp tongue, and a stubborn streak" (p. 6).

Bacon's Mott still follows always the Inner Light. She is a woman whose essential beliefs were simple and lifelong, who maintained "a worshipful attitude toward the good in men and women and in the laws of Nature" and who pursued "unhesitating action in response to Divine Impulse" (p. 226).

Yet Mott's faith was "inextricably bound up into her everyday life" (p. 226). And so her spiritual strength revealed itself primarily through daily action. Like a counterpoint against the steady base of values rooted in her Nantucket girlhood, Mott's life changed as she interacted first with Quaker families in other places, then with reformers in the larger world. In 1833, at forty, Mott entered her most active period of reform. In 1840, her attendance at the World's Anti-Slavery Convention in London "unleashed her," Bacon argues; "thereafter she did not attempt to hold back either anger or commitment" (p. 99). Mott's spiritual growth was not without physical cost, which Bacon measures by Mott's recurrent attacks of dyspepsia. Bacon rightly emphasizes as well the consistent support that Mott received from her family.

Scholars might wish that Bacon had cited sources for general information as well as for direct quotations and that she had identified endnotes by numbers in the text instead of by page and line references at the back of the book. Yet these compromises make *Valiant Friend*, like Bacon's earlier works, appropriate for a popular as well as a scholarly audience.

Bacon has given us a readable, carefully researched narrative of a Lucretia Mott whose spiritual power is especially impressive because it was enmeshed so thoroughly in her daily life.

JUDITH WELLMAN
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Oswego

ROBERT H. ABZUG. *Passionate Liberator: Theodore Dwight Weld and the Dilemma of Reform*. New York: Oxford University Press. 1980. Pp. xi, 370. \$19.95.

At the time Gilbert H. Barnes and Benjamin P. Thomas published their studies of Theodore Weld, psychohistory had scarcely been heard of, and historians did not yet as a matter of course incorporate into their works the insights afforded by psychology and psychiatry. But that was long ago. Now a new

dispensation enables Robert H. Abzug to view Weld from perspectives undreamed of by earlier students of his career. The result is a radically new kind of history and, at the same time, an iconoclastic treatment of one of the acknowledged giants of American reform.

Both Barnes and Thomas were intent on presenting new facts and ideas about abolitionism and on locating the points at which Weld's life intersected with public events. Their subject's shifting emotional states concerned them but little and thus did not occupy the foreground of their works. They portrayed the public, not the private, Weld. Abzug reverses this emphasis.

Passionate Liberator does not serve as a vehicle for explicating the events and movements in which Weld figured, nor does it furnish much new information. Instead the reader finds a sensitive and persuasive depiction of Weld's personality and of the anxieties and spiritual crises that beset his unusually long life. This is one of the rare examples in American historical literature of true biography.

While slavery loomed as an objective evil, Weld's relation to the movement against it, the author argues, was set by his own private needs. The interplay of those needs and his antislavery experiences, which sometimes—as in his defeat at Troy, New York, in 1836—were emotionally pivotal, is the counterpoint that sounds the theme of this book.

The effort to locate within the psyches of individual reformers the dynamo that ran abolitionism certainly did not originate with Abzug. But of all examples of the genre, his commands the greatest respect. Earlier attempts typically suffered either from being unsystematic and unindividualized or else from relying more on insinuation and fortuitous evidence than on systematic research. In their pages cynicism often appeared to supply the sole informing concept. *Passionate Liberator*, in contrast, employs a controlled method and a coherent conceptual structure while showing compassion in examining its subject's inner life. Whether its findings can be extended to embrace more figures than Weld is an important question that no one yet can answer. On that answer depends to a large extent the ultimate significance of this book.

MERTON L. DILLON
Ohio State University

JAMES W. CORTADA. *Spain and the American Civil War: Relations at Mid-Century, 1855-1868*. (Transactions of the American Philosophical Society, number 70, part 4.) Philadelphia: American Philosophical Society. 1980. Pp. 121. \$10.00.

In this volume James W. Cortada purports to enlighten readers regarding a previously "uncharted

area" of Spanish-American relations. In discussing "the most important point of friction" (p. 6) between the two countries, Cuba, he offers little more than a rehash of the Black Warrior affair, the Ostend Manifesto, U.S. filibustering in the Caribbean and Gulf areas, and the persistent, even obnoxious, efforts of the Pierce and Buchanan administrations to secure Cuba during the 1850s. Nonetheless, his accounts of the El Dorado incident of 1855 and American-Cuban commercial relationships during the period, and his insights into the reactions of high officials and leading journalists in Spain to heavy-handed American diplomacy regarding Cuba, appear quite original.

"Spanish penetration into Santo Domingo predated 1861 by many years and with the encouragement of the Dominicans" (p. 30). Moreover, it would appear that historians like Glyndon Van Deusen, who have made much of the American secretary of state's "blustering" instructions to Madrid and "menacing" notes to the Spanish minister in Washington which elicited a firm stand by the Spanish that allegedly forced William H. Seward to back down, may have exaggerated the importance of the confrontation. Cortada indicates that the diplomatic encounter over Santo Domingo may have been more ritualistic than substantial.

Regarding the European intervention of the 1860s in Mexico, Cortada shows both that the venture had been discussed in Madrid as early as 1857 (thus negating the idea that it was part of a hasty scramble to take advantage of the military distractions of the U.S. government) and that "Spain had no master plan for the reconquest of Mexico," merely the protection "of Spanish lives, property, and debt collection," (p. 47). Nor did Spain seriously consider diplomatic recognition of the Confederacy in the absence of prior action to that end by the governments of Great Britain and France.

"Friendlier relations between Madrid and Washington" during the period 1855 to 1868 were "an impossible dream" because both nations "competed for dominance in the New World and each wanted to govern Cuba." A continuation of the earlier "tradition of conflict" was "almost inevitable" (pp. 109-111).

Disconcertingly, Cortada asserts that "American historians have generally believed" (p. 68) or "historians over the years have described" (p. 55) in order to suggest that his own findings refute the interpretations of others, but without citing examples of the aforementioned historians. There are also a few careless factual mistakes (for example, page 47, where the American minister in London is referred to as Gerald F. Adams) and some grammatical errors (pp. 68-69). Moreover, this paperback volume will probably have to be rebound for library use; my copy started shedding pages almost as soon as I picked it up.

Caveats aside, it should be recognized that Cortada is the first historian to make extensive use of Spanish archival sources and Spanish newspapers in treating Civil War-era diplomacy. In addition, although he appears not to have consulted very many private manuscript collections, he has utilized foreign office archives in Great Britain, France, and the United States with admirable thoroughness. This book is an important contribution to the understanding of nineteenth-century American diplomatic history.

NORMAN B. FERRIS

Middle Tennessee State University

OSCAR HANDLIN AND LILLIAN HANDLIN. *Abraham Lincoln and the Union*. (Library of American Biography.) Boston: Atlantic-Little, Brown. 1980. Pp. x, 204. \$10.95.

Americans will never cease to be drawn to the Abraham Lincoln story—fascinated, impressed, and, at the same time, mystified by it. How the poor boy from the backwoods, with little to start with and largely self-educated, could raise himself out of obscurity, in time cultivate an appeal that propelled him into the presidency, and develop a mastery of politics and leadership to guide the nation skillfully through the excruciating turmoil of the Civil War continues to entrance us.

In this compact, straightforward book, historians Oscar Handlin and Lillian Handlin take another look at Lincoln. There is no attempt here at a full-blown, detailed biography. But the Handlins, too, are fascinated by Lincoln's "combination of qualities in counterpoise—strength and frailty, faith and skepticism, rationality and emotion." They find that "the interplay among these attributes" gave Lincoln a "common humanity" that still intrigues us today.

In dealing with his generation's chief dilemmas, the Union and slavery, Lincoln showed a strong sense of history. Americans of his time had to wrestle with "the quiet dogmas of the past," had to "rise with the occasion," and "as our cause is new, so we must think anew, and act anew." And yet believing that "we cannot escape history," Lincoln, after years of troubled reflection, became convinced that the cause of saving the Union and the crusade to end slavery were so intertwined with the long-standing ideals of the American Revolution and with his own sense of justice as to demand positive action. Tragic and terrible as the war was, it could be justified only as a means to preserve the Union and thereby move forward toward achieving the ideals of liberty, justice, and equality and ensuring "that government of the people, by the people, for the people shall not perish from the earth." But more—"In giving freedom to the slave, we assure

freedom to the free. . . . We shall nobly save . . . the last, best hope of earth."

Here, as the Handlins see it, lie the keys to understanding Lincoln's permanence in America's story. Clearly his unmatched skill in eloquently expressing the aspirations of the American experiment gave stirring inspiration to his generation and to those of us who follow after. This modest, well-done volume gives us Lincoln in brief.

DAVID LINDSEY
California State University,
Los Angeles

CLAUDE F. OUBRE. *Forty Acres and a Mule: The Freedmen's Bureau and Black Land Ownership*. Baton Rouge: Louisiana State University Press. 1978. Pp. xvii, 212. \$15.00.

The 1980s may well be the decade of the small farm once more, given the invention of carefully designed programs for owners of as little as twenty-five acres. Booker Whatley of Tuskegee Institute, the discoverer of one such plan, claims that a net income of \$50,000 per year may be generated from 25 acres. This development plus the fact that prime farm land now sells for \$3,500 per acre make all the more relevant Claude F. Oubre's account. Forty acres of good land, rations, tools, seed, fertilizer, and necessary protection for the first few years certainly would have made a difference for the newly freed slave and his descendants as well as for America at large. We might have been spared three generations of sharecropping and worse.

Oubre's account is balanced, comprehensive, and I think fair. No single aspect is ignored. Appropriate space, for example, is devoted to the Reconstruction Congresses, to President Andrew Johnson's motives and policies, to federal laws frequently in conflict, to bureau chief Howard and his subordinates, to white Southerners, and to the newly emancipated. The author's conclusions, including his view of the inadequacy of the Southern Homestead Act, are well argued and compelling. Perhaps most striking among them is mention of limited success upon a canvas of failure: "Yet, despite the failure of the masses, freedmen did achieve considerable success. During the first thirty-five years of freedom, 25 percent of the black farmers of the South acquired land. Since the assistance provided by the bureau only helped to counteract the negative aspects of slavery and in no way altered the prevailing racial attitude, their success represents a personal triumph against overwhelming odds" (pp. 197-98). While I would place this work upon the required reading list for courses dealing with Reconstruction and will inscribe it gladly in my own bibliography, *Blacks in American Agriculture, 1619-1980*, I have but one criticism. It is true that Civil War and Reconstruction

historians have avoided the land question, as the author has pointed out. One who went straight to this issue as early as 1903 was the late William Edward Burghardt DuBois in *Souls of Black Folk* and later *Black Reconstruction in America, 1860-1880*. Oubre should have mentioned him.

JOEL SCHOR
Department of Agriculture

ROBERT H. BREMNER. *The Public Good: Philanthropy and Welfare in the Civil War Era*. (Impact of the Civil War Series.) New York: Alfred A. Knopf. 1980. Pp. xviii, 234. \$15.00.

American historians' interest in philanthropy emerged in the late 1950s after Merle Curti brought to our attention the centrality to social history of this relatively unexplored topic. The assumption was that America's way of giving and ameliorating social needs was unique, suggestive of something distinctive in American character. The impetus stemmed, perhaps, from Americans' consciousness of themselves as being open-handed in giving to the rest of the world and their persuasion that economic distribution was being rectified by philanthropic foundations established to dispense the great fortunes accumulated in the nineteenth and early twentieth centuries.

The scholarly work in the field extended beyond philanthropy and charity to include major studies on professional social work and the development of our social and public welfare systems. Several scholarly reputations have been built in this field, and Robert H. Bremner has been one of the most prolific and influential of these historians, finding articulation of the central theme in studies of poverty and children.

Historians have accepted the view that the Civil War marked a sharp division between the old and the new. While not uncritical of it, we have accepted the contemporaries' view that a sense of community, order, and control was a casualty of the growth and change accompanying the war years. Bremner's book gives concreteness and substance to that assumption by describing both public and private efforts at social service and welfare during the war years. The war made demands that conventional philanthropy was unprepared to meet. It required new agencies such as the U.S. Sanitation Commission and the Freedmen's Bureau. Not merely new—designed to meet specific and extraordinary needs—they were national organizations representing the first major national efforts in welfare. Bremner devotes chapters to each of these activities, to the transformation of private charity and public relief, and to philanthropy as a ladder for the socially aspiring.

He also describes the emergence of individuals of

great wealth seeking means of using their wealth to serve, by their lights, the public good. They were not new; there had been men like Stephen Girard. In the postwar years, however, there were more of them, competing almost to outdo one another in munificence. The range of their interests were national rather than local, and often their gifts were at least in part motivated by a desire to enhance themselves socially.

Bremner has written a readable book that identifies and describes these agents of change. The book, however, is lacking in analysis. Few questions are raised beyond those that would serve description and delineation. One is left to discover the relevance to American social history. Philanthropy assumes a great deal about the nature of community—that mutual obligation of person to person. Community has been problematic in America from the beginning, and at no time more so than in the years covered in this book. Bremner, however, merely tells us what happened in a clear and orderly way.

In passing, he tells of P. T. Barnum's fanciful scheme for "a free museum for the instruction and edification of the Youth of America." It comes at the end of the book, and is to no purpose other than display. It aptly suggests what this study is: a "museum" of thoughtfully selected and well-ordered display. Everything is clearly labeled, but there is little penetration beyond the exhibit.

NATHAN I. HUGGINS
Harvard University

ALICE COWAN COCHRAN. *Miners, Merchants, and Missionaries: The Roles of Missionaries and Pioneer Churches in the Colorado Gold Rush and Its Aftermath, 1858–1870*. (ATLA Monograph Series, number 15.) Metuchen, N.J.: Scarecrow Press and American Theological Library Association. 1980. Pp. xi, 287. \$15.00.

This study has been selected by the American Theological Library Association for inclusion in its dissertation series. It is an example of regional history, wherein the area surrounding what was to become Denver serves as a laboratory to study the general trends of mid-nineteenth-century American religion and does so against the background of frontier history. Instead of offering startling new insights, Alice Cowan Cochran concludes that "Coloradans soon fell into the pattern of the rest of the nation" (p. 171). While challenging the Turner thesis that the environment of the frontier forced changes, suggestions are offered as to how with some adaptations more recent interpretations of American religious history can be documented during this brief Colorado frontier between the discovery of gold (1858) and the arrival of the railroad (1870).

Cochran states that as the surface gold was quickly removed more permanent pioneers settled the area. Among these there was the determination to impose the religious and cultural patterns they had known in their former homes. For the Roman Catholics, largely from New Mexico, and for the Protestants, primarily from the Midwest and East, the story unfolds of how traditional denominations were established by accepted mission methods. In the case of the Episcopalians, Presbyterians, and Congregationalists, it was a continuation of patterns developed on the forested and grassy plains of the Midwest. For the Methodists, who were thought to be more influenced by the frontier, the story is that of the established circuit rider system. Inasmuch as the time frame surrounds the Civil War, there was little representation from the border or Southern states and so a more minimal presence of the Disciples of Christ and the Baptists, denominations frequently associated with the American frontier.

The area and time frame considered do not permit an examination of the tortured routes several denominations were forced to travel as they sought to continue and often adapt time-honored mission patterns to the vast and sparsely settled Great Plains and Intermountain West, whether during the time of the roving cattle herds, the development of fenced ranches, or the era of the homesteader. The strength of the study is that this remote and yet transplanted scene provides an opportunity to analyze American religion, including sermons, revivalism, and social concerns as well as mission ideals and methods in those years immediately preceding the impact of urbanism, industrialization, and the intellectual currents of the last quarter of the nineteenth century.

MYRON J. FOGDE
Augustana College

ELLIOTT WEST. *The Saloon on the Rocky Mountain Mining Frontier*. Lincoln: University of Nebraska Press. 1979. Pp. xvii, 197. \$14.50.

This slender volume describes the saloon and the saloonman in the gold and silver mining camps of the Rocky Mountains between 1860 and 1890. Following the usage of the sources, it deals with numerous public drinking places between Tombstone, Arizona, and Helena, Montana, that for their income predominantly depended on selling liquor by the drink. On occasion the study quite naturally focuses on Leadville, in 1882 Colorado's population center, which epitomized the extremes of geography as well as the social and economic conditions of a mining frontier that produced a distinct kind of saloon. The book presents its characteristics in a matter-of-fact tone that avoids attack or defense of

drinking and shuns nostalgia. The chapters concentrate on delineating the appearance, operation, and economics of the tavern in the light of the functions it served and the person who ran it. The saloon's significance is stressed as a home for footloose workmen, a place of diversion from toil, a male sanctuary, an economic opportunity, and a source of public revenue. These features the study finds most clearly represented in the early stages of the settlement before urbanization turned some mining camps into towns and others into ghost towns, bringing other institutions to the scene or eliminating any need for them.

For his material Elliott West relies primarily on reports of local newspapermen, fortified by diaries, travel accounts, letters, and town records. He uses the manuscript returns of the federal census of 1870 and 1880 to present an appendix that places the race, sex, median age, marital status, and origin of some saloon owners in the context of pertinent samples from the general population of a number of settlements in Arizona, Colorado, Montana, and Idaho. Reminiscences, biographical sketches, and regional histories provide another appendix listing 101 saloon owners alphabetically with tables of their ethnic origins and previous occupations. By placing his findings in the framework of the literature and debate on alcohol and drinking, the author provides a sober picture of the saloon. His concern for detail generally gives a sharp focus to the book, but the developing picture seems somewhat narrow if one considers the work that Rodman Paul (mining), Richard Lingenfelter (labor), and Duane Smith (urbanization) have done to provide a broad context. The captions of twenty-three pictures reinforce the approach rarely employing the haunting images of men, artifacts, or locale to bring out the complexities of people's struggle with loneliness fortified by liquor.

GUNTHER BARTH
University of California,
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MORRIS J. VOGEL. *The Invention of the Modern Hospital: Boston, 1870-1930*. Chicago: University of Chicago Press. 1980. Pp. ix, 171. \$15.50.

Boston hospitals of 1870 were places for the "deserving poor" and others whose circumstances precluded treatment at home to get medical care without being forced into the almshouse. To selected physicians and surgeons they provided an opportunity to improve their medical skills and reputation, which would make them more sought after by paying patients; to trustees and other donors, they provided the chance to exercise their sense of stewardship, to help maintain social stability, and to define,

maintain, and justify their social status. Trustees and medical staff, Brahmins all, were closely linked by family and social ties.

After 1870, political pressure at City Hospital brought changes in rules governing admissions, patients' behavior, visiting hours, and so forth, thus "countering the image of hospital patients as a socially marginal, dependent class" (p. 58). As medicine advanced, hospitals increasingly emphasized medical rather than charitable accomplishments; costs rose, the staff gained more say in management, and the treatment of patients became more medical and less supportive. As more of the "interesting cases" went into hospitals, they became essential in medical education, and the struggle for university control of staff and services became acute. Harvard had to seek recruits from outside the Boston elite if it was to become a national institution. Specialization forced reorganization and emphasis on clinics for teaching and research needs despite older physicians' complaints about "charitable abuse." Urbanization and attendant changes in family structure and living arrangements resulted in the establishment of private hospitals in the 1880s and 1890s, where the middle class could receive noncharitable treatment outside the home. When these could no longer meet the needs of scientific medicine, the charitable hospitals had to find mechanisms to handle the paying private patient. Finally, the change is traced in the shifting sources of funding and the stated purposes for which funds were sought.

Morris J. Vogel's book is a careful and original examination of the hospital as a social institution based on a wide variety of sources and is a valuable contribution. In the introduction he argues that "the march of scientific progress, once fashionable as an explanation for changes in medical practice and organization, is insufficient" (p. 4). Other characterizations of hospitals and medicine by writers like Ivan Illich are now more fashionable among some, and occasionally Vogel seems to reflect their antimicrobial attitude. His work makes clear that a variety of nonmedical considerations affected the organization, funding, and management of hospitals, but in the end he reinforces the view that the essential impetus for change did indeed come from the development of medicine, with obvious benefits to the patient population.

JOHN B. BLAKE
National Library of Medicine

GWENDOLYN WRIGHT. *Moralism and the Model Home: Domestic Architecture and Cultural Conflict in Chicago, 1873-1913*. Chicago: University of Chicago Press. 1980. Pp. viii, 382. \$17.50.

Moralism and the Model Home studies ideas about domestic architecture in Chicago from the period of the domination of Victorian ideas of domesticity to "modern," progressivist notions about the home and family, with their realization in the suburb. It is in the model, or typical, middle-class house that these ideas are most clearly reflected, especially in relation to a complex rhetoric of morality that smooths over the ragged edges of complex and competing values.

Gwendolyn Wright bases her discussion of architectural ideas firmly within the socioeconomic history of the period and demonstrates the power of cultural movements to affect the conception of the model home. As a result, she allows us to see how model domestic architecture underwent modification in reaction to such social changes as the development of public transportation, the home economics movement, and the germ theory of disease, responsible for the sleeping porch and the sun room and for turning American interiors white, "the sign of visible sanitary awareness" (p. 119).

Appropriately, and refreshingly, Wright turns to sources generally undervalued, if not entirely overlooked, primarily architects' books and journals, builders' publications, pattern books, domestic guides, and home magazines. She is expert in her use of these materials, understanding clearly how "editors of popular magazines for women and their families used housing descriptions to reinforce political statements about class inequalities, the need for community, and citizen participation" (p. 148). Wright argues correctly that in a fluid society "the 'middle-class family' was recognized by home ownership" (p. 83); moreover, that "to be a householder is to be a citizen" (p. 294).

Wright's concentration on the middle-class house necessarily forces onto the periphery consideration of the housing of the rich and the poor, housing that in both cases responds to somewhat different values and tensions. While it can certainly be argued that the poor aspired, or were supposed to aspire, to the housing of the middle class, the fact remains that the failure of the poor, and especially the immigrant poor, to realize these aspirations fueled the fires of nativist criticism. At the same time, the extraordinarily ostentatious turn-of-the-century houses of millionaires raised serious questions in the minds of both social and architectural critics about the American economic system. In both cases, the effect was to reify the middle class house as the symbol of conservative American values.

Unquestionably, domestic architecture in America provides a rich source for the social historian; as Wright says, "There is something peculiarly American about the campaign to define and publicize models for the home and to connect those models to

other, larger social goals" (p. 293). Her study, with its emphasis on the documents that discuss, conceptualize, analyze, and criticize the American house, is a rich and thorough work in this important field.

JAN COHN

George Mason University

STEVEN J. DINER. *A City and Its Universities: Public Policy in Chicago, 1892-1919*. Chapel Hill: University of North Carolina Press. 1980. Pp. 263. \$18.00.

Focusing primarily on professors from the University of Chicago and Northwestern University in the late nineteenth and early twentieth centuries, this book analyzes the contributions that scholars made to solving some of Chicago's social problems during that period. While the major concern of the book is with Chicago, Steven J. Diner demonstrates with comparative materials that a similar process was occurring in Baltimore, Boston, New York, and Philadelphia.

Using a wealth of data on the elites of Chicago, the author demonstrates that a sizable percentage of Chicago's reform leaders were university professors. Moreover, he presents data demonstrating the extent to which members of each academic department at the University of Chicago and Northwestern University were involved in urban reform.

The Chicago professors were active in reforms involving the city's educational system, urban planning, the juvenile court system, vocational guidance, an employment bureau, public health, and home rule. Diner demonstrates that the professors' reform activity led them to become involved in an alliance with upper-class groups who advocated similar goals, an alliance that was often at variance with the goals pursued by lower- and middle-class elements. The author's study is not strong on class analysis, though he is careful to emphasize the fact that most of the reforms tended to benefit the interests of the upper class. Nonetheless, this reviewer wishes that the author had provided a better understanding of the symbiotic relationship between the reform activities of university professors and the dependency of the universities on the economic elites of Chicago. Were there frequent strains between the goals of Chicago's economic elites and its university professors? Other than memberships in common organizations, what kind of social interaction existed among university professors and other reform leaders of Chicago? Answers to these questions would help us to understand the extent to which there was a class consciousness on the part of Chicago's reformers.

While the author has provided a useful description of the role that university professors played in promoting reform in Chicago, New York, Boston,

and Philadelphia, this reviewer is intrigued by a question, the answer to which may have required the author to go beyond the concerns of this particular study. Even though experts within the universities contributed to reform activity, were they, from a theoretical point of view, essential to the reforms that emerged in the response to industrialization? In other words, how important were the universities and the writings of university professors for the types of reforms that concern the author? Certainly many cities eventually acquired many of the same types of reforms even though universities and university professors played little role in their presentation. Examples of such cities are Cleveland, Milwaukee, Portland, and San Francisco. Was there a different historical process in these cities and the ones studied by Diner? Or, were university professors and universities relatively unimportant as an *explanation* for the transformations that occurred in either of the two sets of cities? The answer to this question is not easy and would have required a different research strategy from that which the author employed.

Within the confines of what he set out to do, however, Diner has written a very good book. The research is impressive, based on numerous collections of papers, as well as on a thorough use of primary and secondary sources.

J. ROGERS HOLLINGSWORTH
University of Wisconsin,
Madison

SHERRY H. OLSON. *Baltimore: The Building of an American City*. Baltimore: Johns Hopkins University Press. 1980. Pp. ix, 432. \$22.95.

This work is a patchwork of anomalies. Substantive scholarship, punctuated occasionally by brilliant insights, is packaged in a coffee-table, picture-book format that does not quite succeed at its genre. The eleven chapters are unnumbered, range unevenly in length between ten and fifty-seven pages, and follow one another in unexplained chronological order. There is no bibliography, and its multitude of black and white illustrations have captions but no table or index to them. In defense of the package, the notes are extensive and the two indexes, one of the subjects and the other of people, are detailed and easy to use. Also, the entire volume is remarkably free of typographical errors and the text, being descriptive rather than analytical, is easy to read and chatty in tone. Jargon only rarely shows itself: "The downtown area was rebuilt [during the 1840s] for higher throughput" (p. 111).

Baltimore is used as a prism in this study. Sherry H. Olson's main concern is with "city building, an internal dynamic of the city-state, and how Balti-

moreans see themselves and their situation" (p. ix). She uses a boom and bust cycle that measures about eighteen years between peaks and that, she says, characterized the development of urban North America and Europe as well as Baltimore during the past two centuries. The book is constructed upon this cycle, and seven of the eleven chapters follow this rhythm with varying degrees of success.

Whether one agrees with the cycle or not, the structure is a major weakness of the book. We have no controlling or unifying themes of urban, or even Baltimore's growth that link the chapters. Nor do we have a summary chapter that provides an overall assessment or conclusion about Baltimore's historical experience. The seventh and eighth chapters, "A Rent in the Social Fabric, 1866-1877" and "Consolidation, 1878-1899" are the extreme cases of meticulous description uninformed by analysis. The reader is not told what was rent from 1866 to 1877, or why, or what was then consolidated and why.

Indeed, many of the chapter titles suggest ideas that remain unexplained in the text. "Hemmed In, 1935-1979," the title of the last chapter, simply mystifies me, given Baltimore's renaissance during the 1970s.

But all is not negative. The strengths of this study lie in its diverse research, its enormous fund of information about Baltimore, and its interesting interweaving of economic, geographic, and social patterns of development. Indeed, Olson's handling of these interdisciplinary matters and her characterization of historical moments sometimes verge upon the brilliant. Urban specialists, particularly those concerned with systems analysis, should find Olson's insights into the urban historical process provocative and rewarding.

GARY L. BROWNE
University of Maryland,
Baltimore County

JAMES BORCHERT. *Alley Life in Washington: Family, Community, Religion, and Folklife in the City, 1850-1970*. (Blacks in the New World.) Urbana: University of Illinois Press. 1980. Pp. xiv, 326. \$18.95.

This book examines a type of residential segregation formerly found in certain Eastern cities where blacks often lived in inferior alley dwellings behind white residences. James Borchert, through manuscript census materials, building permits, and fire insurance maps traces the origins and evolution of this housing pattern and describes the social institutions and life-styles of the poverty-stricken blacks dwelling in these enclaves. Drawing upon earlier investigations by social reformers, the author argues that their middle-class value judgments resulted in

conclusions that the blacks lived pathological and disorganized lives. But Borchert—heavily influenced by contemporary urban anthropology—has skillfully illuminated how these alley dwellers forged their own culture, enabling them to adapt creatively to their harsh existence. Thus he stresses the black residents' "integrated philosophy and world-view that resisted the segmentation of the city," their kinship network, and their alley community that "provided protection, support, and friendship for its occupants."

While Borchert's corrective to the earlier interpretation is welcome, despite his disclaimer he leans too heavily in the other direction and tends to so understate social problems of the impoverished alley dwellers that their lives are made almost idyllic. The suffering and hostility that alley residents sometimes inflicted upon each other receive little attention. For example, male lodgers, rather than sexually exploiting growing girls as they sometimes did, are all depicted as people "incorporated to expand the family's network for support in difficult times." Moreover, on the basis of very limited data, Borchert makes generalizations about juvenile delinquency and violent crime, again to the point of scarcely recognizing their existence.

Yet this book's many virtues include its fresh approach, its very lively writing style, and its brilliant use of photographs that are marvelously integrated with the text in a manner that this reviewer has seldom seen. The thirty-five photographs of exteriors of the alley community as well as alley house interiors (culled from seven hundred photographs) bring a vivid physical perspective to the way of life of the inhabitants. Readers will find unforgettable photographs showing the alley dwellers with such furnishings as oil lamps and "trunkbeds," doing arduous tasks such as laundering for a living (in which the washerwoman carried into her kitchen cold water from a backyard hydrant, which was dumped into a large bucket and heated on a coal stove), and "junking" (by which many a resident survived by collecting and selling large quantities of rags, paper, scrap metal, tin cans, and broken glass).

This highly original contribution will prove an unusually useful book for classroom teachers, and one hopes that it will soon appear in a reasonably priced paperback.

ELLIOTT RUDWICK
Kent State University

MELVIN EASTERDAY DIETER. *The Holiness Revival of the Nineteenth Century*. (Studies in Evangelicalism, number 1.) Metuchen, N.J.: Scarecrow Press. 1980. Pp. x, 356. \$17.50.

In the preface to his book Melvin Easterday Dieter modestly refers to it as a summary of his doctoral research. It is just that—his dissertation, which, one

suspects, has been revised only slightly, if at all, and apparently without benefit of professional editorial attention. The result is an undistinguished study marred by numerous spelling and typographical errors as well as awkward and unclear phrasing.

The Holiness Revival of the Nineteenth Century treats the movement that began among Methodists in the 1830s and preached the necessity of "a second crisis of evangelical faith" following the initial conversion experience. The "second blessing," as it was called, "involved the Christian's utter consecration of himself to God through Jesus Christ in the faith that God would free him from the inner disposition to willful sin and fill him with divine love" (p. 4). The movement spread to other denominations in the pre-Civil War years. After the war it generated increasing controversy within the churches with the result that in the 1880s some holiness advocates began withdrawing and forming separate churches.

Dieter's study raises some interesting questions about the nineteenth-century holiness movement but fails to deal with them adequately. Why, for example, were Southerners uninterested in holiness doctrines before the Civil War but receptive to them after it? What was it about the holiness movement that attracted so many women to its ranks and permitted them to assume leadership positions in it? What is the significance of the holiness advocates' emphasis on the "interior life" at a time when other religious groups, and Americans generally, were inclined toward social activism of one kind or another? Finally, and perhaps most importantly, what generated and sustained the movement during the nineteenth century? Was it fear of social change or a desire to reform the church—to "Christianize" Christianity? Was it what Dieter calls "pietistic revivalism's inherent tendencies towards perfectionism" or the American dream of "individual, national and world redemption which would usher in a Christian millennial order" (pp. 61, 96)? Dieter offers all of these as possible explanations, but he does not develop a convincing argument for any of them. Readers are left to formulate their own interpretations of the movement. The book may prove helpful to those who wish to become familiar with the key personages, events, and doctrines of the revival, but those looking for critical insights regarding it will probably be disappointed.

ANNE C. LOVELAND
Louisiana State University

BRUCE F. CAMPBELL. *Ancient Wisdom Revived: A History of the Theosophical Movement*. Berkeley and Los Angeles: University of California Press. 1980. Pp. x, 249. \$12.95.

Theosophy has generally attracted hypercritical abuse from its opponents, uncritical defense from

Theosophists. From Bruce F. Campbell, however, it receives a sympathetic yet balanced treatment that recognizes the movement's "problems and inner conflicts" as well as its "contributions" (p. viii).

Beginning with the fateful meeting of Helena Blavatsky and Henry S. Olcott in 1874, Campbell charts the Theosophical Society's course: its growth from a New York study group concerned with occult phenomena and arcane "wisdom" to a flourishing international organization after the two founders took its headquarters to India in 1878; its years of struggle over leadership and mission following Blavatsky's death in 1891; and its eventual division as a result of these struggles into three groups whose fortunes Campbell traces to the present—the Adyar Theosophists centered in India (long led by Annie Besant), and the smaller Theosophical Society of America and United Lodge of Theosophists, both anchored in California. Along the way there are helpful critical accounts of Blavatsky's writings and of major Theosophical teachings, and there is a full bibliography.

Among "contributions," Campbell emphasizes the reformulation of Western occultism in Asian terms with which Blavatsky provided an esoteric alternative for those repelled by conventional faith and science, the Society's support of reform and nationalism in India and Ceylon, and its pioneering role as two-way cultural bridge between East and West. The "problems" he touches on include the controversies over Blavatsky's "inspired" authorship and occult feats, institutional troubles with succession and faction, and difficulties in reshaping doctrine and direction in contemporary terms. Today, he notes, there is renewed interest in Theosophy, yet the reciprocal intellectual and religious traffic between East and West that it helped open up no longer requires its efforts.

Campbell's book has its own problems. Though admirably detailed on leadership styles and crises, its narrative is often summary, its sociological analysis sometimes pedestrian. Nor do its sketches of larger historical contexts make a virtue of simplicity. A richer if briefer interpretation of Theosophy's Oriental quest will be found in Robert S. Ellwood, Jr.'s *Alternative Altars: Unconventional and Eastern Spirituality in America* (1979). Yet whatever its shortcomings, *Ancient Wisdom Revived* is the best—indeed, it is the first—scholarly guide to the institutional history of one of the more fascinating religious movements of the last hundred years.

HOWARD KERR
University of Illinois,
Chicago Circle

FREDERIC TRAUTMANN. *The Voice of Terror: A Biography of Johann Most*. (Contributions in Political Science, number 42.) Westport, Conn.: Greenwood Press. 1980. Pp. xxv, 288. \$25.00.

Of all the major movements of social reform, anarchism has been subject to the grossest distortions of its nature and objectives; and of all the expositors of the anarchist creed, none has been more abused and misrepresented than Johann Most, the German-born revolutionary firebrand, who spent the last quarter-century of his life in the United States. Most, it would be no exaggeration to say, was the most vilified social militant of his time. Portrayed in the daily press as a wild revolutionary fanatic, bent on chaos and destruction, he became the cartoonist's stereotype of the bewhiskered, foreign-looking anarchist, with a bomb in one hand and a dagger or pistol in the other. In an editorial published after his death in 1906, the *New York Times* called him a "mad dog" and an "enemy of the human race."

That Most was an uncompromising agitator, an apostle of revolutionary violence and propaganda by the deed, cannot be denied. And yet, he was far from being the rabid, maniacal figure of caricature. On the contrary, he possessed considerable dignity, erudition, and charm. To Emma Goldman, whom he helped to convert to anarchism, he was a highly sensitive individual whose sympathy for the laboring poor furnished the mainspring of his revolutionary energy. A gifted orator and journalist, he drafted the Pittsburgh Manifesto of 1883, which became the charter of the social revolutionary movement in the United States. His celebrated paper, the *Freiheit*, which he edited for twenty-seven years, acquired a place in the front ranks of German revolutionary literature, and decades after his death his powerful "Song of the Proletarians" continued to be sung by German workers of every radical denomination, in Europe as well as America.

An indefatigable propagandist for nearly forty years, Most was an international figure whose influence was felt in Switzerland, Austria, and Germany, as well as in England and the United States. He has long merited a scholarly biography, based on the full range of available sources. Frederic Trautmann, in his interesting contribution, falls short of providing such a work. Relying exclusively on printed materials, he has not availed himself of the rich anarchist archives in Amsterdam and at the University of Michigan. Nor has he examined the German state archives, used to advantage by such scholars as Andrew Carlson and Ulrich Linse. As a result, his biography is incomplete. Most's personal life, for example, receives inadequate attention. Thus his wife's first name was Helene, not Helen, and we are never told the names of his two sons (Lucifer and John, junior), let alone anything about them. Such important friends and associates as Urban Hartung, Tobias Sigel, Max Baginski, Otto Herrmann, and August Lott are mentioned briefly, if at all.

Beyond this, further criticisms are in order. The book's sensational title, *The Voice of Terror*, is misleading, for it suggests only one side of Most's char-

acter and career. Adding to the melodramatic tone is the author's breathless, staccato style, with short sentences and paragraphs and excessive rhetorical devices. The organization of the book, an unsuccessful blend of chronological and topical arrangement, makes the story of Most's life harder to follow than necessary, a problem compounded by transposed pages and lines (pp. 24, 25, 51) and by a disturbing number of factual and typographical errors.

For all its shortcomings, *The Voice of Terror* manages to convey some of the flavor of Most's personality, and it provides useful paraphrases of Most's writings and speeches. It does not, however, supersede the German biography of Most published by Rudolf Rucker in 1924, a English translation of which is long overdue.

PAUL AVRICH
Queens College,
City University of New York

DAVID R. CONTOSTA. *Henry Adams and the American Experiment*. (Library of American Biography.) Boston: Little, Brown. 1980. Pp. viii, 159. \$10.95.

Henry Adams continues to intrigue students of the American past. He has been the subject of at least eighteen scholarly monographs, the most complete being the three-volume study by Ernest Samuels. In this slim volume, David R. Contosta has succeeded in writing a lucid, interpretive book intended primarily for use by undergraduates. As a theme, Contosta explores how Adams conceived and reflected upon the "meaning of the American experiment" as revealed in Adams's prolific writings and personal correspondence. Contosta concludes that Adams believed the experiment rested upon beliefs in the eternal truths of natural law and an optimistic appraisal of human nature. But the twin pillars of the experiment collapsed.

The historical experience of the United States in the nineteenth century revealed the fallibility of human nature, and modern science destroyed the empirical foundations of natural law. Driven increasingly to a cataclysmic interpretation of the nation's destiny, Adams, unlike most of his contemporaries, saw the United States (and the West generally) in the unrelenting grip of social and cultural devolution rather than progressive evolution.

While Contosta necessarily resorts to the compression and simplification of the ideas of Adams, undergraduates should find his study both provocative and disquieting. Mature scholars are also likely to find it rewarding.

BENJAMIN G. RADER
University of Nebraska,
Lincoln

BRUCE SINCLAIR. *A Centennial History of The American Society of Mechanical Engineers, 1880-1980*. Toronto: University of Toronto Press, for the American Society of Mechanical Engineers. 1980. Pp. xii, 256. \$15.00.

Bruce Sinclair has written a model of organizational history. The American Society of Mechanical Engineers sanctioned and published Sinclair's study, but it is definitely not an "official" history. Unlike most of that genre, it neither abstracts the organization from the larger history nor requires the reader to possess a photographic memory to keep track of the names.

The Society's formation was a part of the developing infrastructure of modern business capitalism. Its founders wished to create "institutions needed to systematize the flow of information from research and experience to practical application" (p. 25). Not long removed from the machine shop, they wished the ASME to provide "the matrix for a new social class" (p. 61) of professionals distinguished from machinists. By systematically gathering and making data available, they believed they would at once further the quality of modern civilization and enhance the welfare and status of engineers.

Sinclair's study allows us to better understand the role played by mechanical engineers as supporting actors in industrial capitalism's search for orderliness and systematic procedures. Within its first decade of existence, the ASME developed a means of establishing technical standards that "was to the engineer what administration was to the manager" (p. 50). The scientific management movement, spawned in the ASME, provided the conceptual framework for the rational organization of work in the twentieth century.

Internal contradictions prevented the society from achieving an independent professional status. Its members sought a distinctive professional identity but acted as agents of industrial power. A national organization, it was controlled by a New York clique out of touch with the needs and interests of members. It aimed at uniting members in a fraternal spirit, but local engineering societies better served its members' social needs. As a technical society, it failed to develop an effective policy for dealing with the centrifugal tendencies of technical specialization among its members.

Sinclair's detailed work augments a growing body of critical engineering history, including works by Monte A. Calvert, Edwin T. Layton, Jr., and David Noble, among others, that allows us to understand the place of engineers in American industrial development.

WILLIAM E. AKIN
Ursinus College

SAUL ENGELBOURG. *Power and Morality: American Business Ethics, 1840-1914*. (Contributions in Economics and Economic History, number 28.) Westport, Conn.: Greenwood Press. 1980. Pp. xvi, 181. \$22.50.

The field of business ethics is a notoriously amorphous and intricate subject of study. Saul Engelbourg's effort to nail this particular bowl of jelly to the wall is more successful than most. Still, as he acknowledges, the problems are difficult, perhaps inherently frustrating. Inevitably, then, his analysis leaves many issues unresolved and some largely unexplored, although he provides a helpful set of baselines and guides for further exploration.

He begins by selecting five specific issues for close investigation: conflict of interest, restraint of trade, competitive tactics, stock watering, and financial reporting. He then attempts to assess changes in thought and practice concerning each of these issues through three periods, spanning nearly a century of American business, in successive chapters: "The Emergence of Power and Restraint, 1840-1880"; "The Federal Government as Regulator, 1880-1900"; and "The Age of Confidence, 1900-1914." Drawing upon his extensive reading of testimony before state and national investigating committees as well as the materials and findings of recent scholarship, he concludes cautiously that the years in question did see significant improvements in the level of business morality as businessmen and the public generally struggled to understand the nature and implications of the new organizational relationships accompanying industrialization and large-scale, increasingly interdependent operations. And he considers a number of sources or motives for change, particularly emphasizing public criticism and regulation; the dependence of large and intricate enterprises upon adequate, accurate information, honest dealing, and lengthened time horizons; and the personal examples and influence of a few especially sensitive business leaders. Among these influences, he tends, with some hesitation, to give primacy to the first, concluding that internal incentives for change would have been insufficient in the absence of external pressures and controls.

Yet there are problems with this analysis, as Engelbourg recognizes. Both internal and external pressures were more successful in limiting some practices than others. Restraint of trade, he finds, was less effectively brought under control than the other practices. Some industries lent themselves to regulation more readily than others. Another difficulty, which he notes but fails, I believe, to resolve is that of distinguishing more sharply between rhetoric and practice. Despite a stated intention to emphasize "perceptions" and "aspiration" more than behavior, the study fortunately—in my view—does not hew consistently to this line. More detailed con-

centration upon actual behavior in many firms, industries, regions, and relationships may further clarify the key issues. A final difficulty involves the perennial pitfalls of periodization: the limitations of Engelbourg's scheme are suggested by the interchangeability of some, at least, of the titles and time periods of the chapters previously mentioned.

Still, this study points in a direction that augurs well for further research, toward more controlled examination of actual behavior in specific institutional and historical contexts.

MORRELL HEALD

Case Western Reserve University

GARY JOHN PREVITS and BARBARA DUBIS MERINO. *A History of Accounting in America: An Historical Interpretation of the Cultural Significance of Accounting*. New York: John Wiley and Sons. 1979. Pp. xii, 378.

This book was written primarily for accounting professionals rather than historians. It is less comprehensive than the main title implies, since Gary John Previts and Barbara Dubis Merino discuss almost exclusively the rise of public accounting (CPAs) and largely neglect managerial or cost accounting. The subtitle, which promises an investigation of the cultural significance of accounting is likewise inappropriate, and given the nature of this study, I found its choice unfathomable. Three important themes are the evolution of theory and practice, the rise of the profession and legal recognition of its special status, and finally the role of accountants as auditors and interpreters of financial statements issued by businesses. They do not discuss, however, the structure of the accounting industry nor attempt to explain why only a few firms—the so-called Big Eight—came to dominate the public accounting field; it is an inexplicable omission.

Business and political historians conducting research on financial reforms in the twentieth century will be interested in the detailed review of the impact of regulatory agencies such as the SEC on accounting practitioners. The authors reveal how the growth of government and its greater involvement in the economy created a host of new opportunities for accountants and how the profession has become more specialized, in the same fashion as medicine or history. CPAs are unique among professional groups because their responsibilities extend not only to their clients but also simultaneously to the public at large, and the book illuminates very effectively the tensions and conflicts that arose—and persist—in trying to serve two masters.

The early chapters are marred by an almost amateurish effort to integrate accounting history into a broader panorama, but Previts and Merino are

much more skillful after they narrow the focus to familiar topics about halfway through the book. As primarily business historians, they are generally successful. The extensive bibliography is praiseworthy.

EDWIN J. PERKINS

University of Southern California

MICHAEL BEZILLA. *Electric Traction on the Pennsylvania Railroad, 1895-1968*. University Park: Pennsylvania State University Press. 1980. Pp. 233. \$16.75.

The Pennsylvania Railroad, which at one time referred to itself as "the Standard Railroad of the World," has played a significant role in the history of American transportation. Michael Bezilla has examined one phase of this corporate giant's many technological advances in his study of its electrification, which eventually resulted in the most extensive main-line electric operations in the United States.

Relying heavily on engineering records of the company and technical journals of the day, the author traces the growth of the Pennsy's electric lines from their early beginnings to their ultimate extent, with catenaries strung between New York and Washington and from Philadelphia to Harrisburg, plus several secondary lines.

The construction of the Hudson River tunnels and Pennsylvania Station in the city of New York forced the Pennsylvania to consider electrification for heavy main-line service. In layman's terms, Bezilla weighs the relative merits of A.C. versus D.C. power and discusses the evolution of locomotive and multiple-unit car design.

The successes of electrification, despite its high initial cost, led the railroad to consider expanding it. Philadelphia suburban lines were electrified next. An installation over the mountains west of Altoona was studied but finally rejected. Instead, the company decided to complete the gap between the New York and Philadelphia operations. By the early 1930s, plans were made to extend the wires to Washington and Harrisburg. The financial burden was relieved somewhat by Reconstruction Finance Corporation loans, which the carrier quickly repaid, but the basic expense was borne by the Pennsylvania. The installation has proven itself many times since, and it continues to serve travelers and shippers today.

This is a fascinating book. The author has done an excellent job of analyzing and appraising the technological and managerial elements in this vast undertaking. The problems of electric traction, the continuous and often unsuccessful efforts to develop better locomotives, the financial pressures, and the railroad's impact on the electric power industry are treated. There are ample illustrations and several

maps. Bezilla feels that the Pennsylvania was more conservative than pioneering in its application of electric traction, especially in the early stages, but that it made sound business and financial decisions when it did act. The footnotes unfortunately are in the back of the book, an arrangement more convenient to the publisher than the reader. The section on World War II could perhaps have been expanded to better demonstrate the value of electricity during that hectic period of the railroad's history.

Overall, Bezilla has done a commendable job. He writes clearly and concisely, and he handles the sources well. His book adds considerably to our knowledge of corporate decision making, technology, and "the Standard Railroad of the World."

JAMES N. J. HENWOOD

East Stroudsburg State College

CLARK C. SPENCE. *The Rainmakers: American "Pluviculture" to World War II*. Lincoln: University of Nebraska Press. 1980. Pp. x, 181. \$15.95.

This book, brief though it is, represents the most substantial study of the American phase of a long-continued effort to produce rain when needed. Understandably, this story is given short shrift by historians of meteorology and of the U.S. Weather Bureau. It is largely an account of hucksters and theory-deficient experimenters who applied hunt-and-try methods wildly. It is the static that good reductionist historians have eliminated in order to bring out stories of progressive improvement in man's ability to track and understand the weather.

Yet these attempts and controversies were also a part of the fabric of history. Rainmaking may look like a pseudoscience that failed, without producing useful offshoots as alchemy, for example, did. It is not, however, an alien scene. The attitudes and methods of its practitioners were congenial with going empirical technologies of their time.

Clark Spence's sources reveal something of the scene. He found no major collection of "pluviculture" papers but put together the account primarily from an enormous range of newspaper and periodical citations collected over many years. The records of the Weather Bureau were helpful, as were a few personal papers. The primary reliance had to be on what the practitioners and their opponents said in public and what they did.

Spence begins his story with James P. Espy, a recognized antebellum scientist who made contributions to meteorology and who advocated building enormous fires to precipitate rain. It ends just before the first successful cloud seeding of 1946, based upon a new knowledge of the physics of clouds and of drop formation. Between, he organizes the con-

tinuing circus in lively fashion. His cast includes a "Storm King," a "Rain King," "Rainmaker Jones," and the "First Lieutenant to Jupiter Pluvius." A sequence of different approaches was tried. Spence classifies them as rainmaking by concussion, smell, chemical affinity, electricity, and sprinkling the clouds. Had it been possible to evaluate the results more accurately, the scope of all these efforts would have been much contracted.

This study presents an aspect of the past neglected because, for most historians, it remains a part of the background noise, not a subject upon which they wish to dwell. Yet it must be integrated with our other studies of the time and the culture to provide any approximation of the complete historical fabric.

BROOKE HINDLE
Smithsonian Institution

FREDERICK W. MARKS III. *Velvet on Iron: The Diplomacy of Theodore Roosevelt*. Lincoln: University of Nebraska Press. 1979. Pp. xiv, 247.

This is a thoughtful reassessment of Theodore Roosevelt's diplomacy, a series of interpretive essays designed to provide a broad framework for our understanding of Roosevelt's foreign policy. Frederick W. Marks III believes that historians have too often mistaken "the personality for the man" (p. 144) and exaggerated Roosevelt's bellicosity and impetuosity. He portrays Roosevelt as a cautious and patient diplomat, one who was sensitive to American public opinion as well as to the viewpoints of other peoples and who forged a rare combination of realism and idealism. Reminding us of the context within which Roosevelt worked, he emphasizes the intense nationalism of the time, the widespread acceptance of imperialist assumptions, and the moral convictions that influenced his diplomacy. What emerges is a flattering portrait of a high-minded, cosmopolitan statesman.

Marks also examines controversial events such as the Alaskan boundary dispute, the Venezuelan crisis of 1902-03, and the Panamanian Revolution. In each case, he defends Roosevelt's behavior and veracity, arguing that he proceeded with patience and sought to avoid embarrassing his adversaries. Marks claims that Roosevelt did in fact threaten Germany with war unless it arbitrated its dispute with Venezuela. Although he does not provide any fresh documentation, his argument is ingenious, and his explanation of the gaps in official American, British, and German records is imaginative. He is impressed with Roosevelt's patience in dealing with Colombia, defends the legitimacy of the Panamanian Revolution, and, all in all, makes a persuasive case for American policy.

It is useful to learn more of Roosevelt's positive, constructive characteristics, although Marks goes too far, creating a statesman without a serious flaw and even dismissing Roosevelt's fascination with violence and war. His excessive admiration of Roosevelt, along with his tendency to get bogged down in old historical disputes, lessen the effectiveness of the book and of the interpretative framework that it seeks to create. Like Howard K. Beale, Marks is determined to establish Roosevelt's veracity, to prove that he "can be trusted on matters of fact" (p. 69). But all men in power—to one extent or another—distort the events of which they were a part, and Roosevelt was no exception. The excessive criticism of his detractors should not be met with excessive praise from his admirers. We need a book that moves beyond the old disputes and that analyzes Roosevelt's diplomacy in a different way, one that explores his geopolitical fantasies, that locates him within the spectrum of American imperialist thought, and that explains his peculiar approach to the organization of American diplomacy. Rather than rehashing tired controversies, such a study would break new ground and perhaps stimulate a major reassessment of Theodore Roosevelt and his approach to world affairs.

CHARLES E. NEU
Brown University

JOHN F. MCCLYMER. *War and Welfare: Social Engineering in America, 1890-1925*. (Contributions in American History, number 84.) Westport, Conn.: Greenwood Press. 1980. Pp. xvi, 248. \$22.95.

A reviewer impelled toward a sermon on the need for charity in historical judgments must be charitable. John F. McClymer's *War and Welfare: Social Engineering in America, 1890-1925* has most of the attributes of scholarship. It is well and clearly written. It rests on extensive research, largely in the archives of charitable institutions. It presents a straightforward interpretive thesis and tests it with historical evidence. The work's merits warrant a respectful reading.

A summary of *War and Welfare* risks little distortion, for McClymer has presented his thesis starkly, with few of those qualifications the actual confusion of events forces on most historians. Part 1, "The Emergence of Social Engineering," analyzes how workers in city settlement houses in the United States around 1900 began to take on the attributes of professionals. He then details how a number of these budding experts came together in 1907 to explore urban realities through the "Pittsburgh Survey" and how, out of the massive compilations of that investigation, they evolved new techniques of "social accounting" that implicitly accepted the

folkways of corporate capitalism. Finally, he evaluates the relationship, largely unsatisfactory, of the Survey group with the Russell Sage Foundation. Part 2, "The Quest for Power," gets McClymer to the heart of his thesis. He describes, rather brilliantly, the conflicting views on immigrants of the welfare professionals, dividing the latter into those who focused on the immigrants' economic impact, the "racialists" who emphasized the problems rather than the promise of ethnic diversity, and the "cultural hegemonists," the dominant group, who promoted assimilation for the foreign born. The rest of the book then takes on the tone and substance of a morality play. The "cultural hegemonists," McClymer argues, became fatefully inclined before and during America's involvement in World War I to shift from paternalistic programs of assimilation to chauvinistically coercive plans for Americanization. They shifted, at the same time, their professional aspirations from the foundations to wartime government agencies. The experts are alleged to have had a naive and self-serving confidence that their expertise would give them actual control over society. Their comeuppance amid the passions of war and postwar reaction is preordained.

The tale has warrantability for some of McClymer's protagonists, particularly Frances Kellor and Edward T. Devine, but he acknowledges only in grudging footnotes that the prominence of both may have reflected more what American society at the time wanted than any real representativeness or leadership of the social work movement. His most reiterated theme is that the social engineers almost invariably adopted the views and techniques that would most serve their own professional advancement. Such a charge would be hard to document whatever the research; McClymer, relying almost entirely upon public and institutional records, presses his case through thinly supported inference or views he finds "implicit" in selected statements. The advocates of organized charity do not deserve to have been dealt with so uncharitably—and ahistorically.

CHARLES FORCEY
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ROBERT L. ZANGRANDO. *The NAACP Crusade Against Lynching, 1909-1950*. Philadelphia: Temple University Press. 1980. Pp. ix, 309. \$19.50.

While reading Robert L. Zangrando's *The NAACP Crusade Against Lynching, 1909-1950*, one realizes that the present time bears many similarities to the period under study. There is widespread violence against blacks (in Atlanta, Boston, Buffalo, and elsewhere) that appears to be part of a national

campaign. Then there is the rise and growth of the Ku Klux Klan and the American Nazi party. And, finally, there is the incoming administration of President Ronald Reagan, which is made up of people not especially sympathetic to the aspirations of black people.

Zangrando has written an excellent history of the NAACP's crusade against lynching that spanned more than four decades. Although the author maintains that the book deals with the role of the NAACP in the crusade, and the consequences of the struggle for the organization and for the subsequent civil rights movement, by placing the events in the context of the times he succeeds in writing a social history of the period and of the organization. It is indeed a depressing account.

The NAACP did not succeed in its campaign to secure a federal antilynching law, and that it did not illustrates the depth of American racism. The author covers many other struggles engaged in by the NAACP: discrimination in the armed forces, in higher education, in public transportation, in housing, and in voting. With little sympathy from presidents and members of Congress, and with persistent need for financing, social change in the realm of race relations made little headway in the first half of the century. The long drive against lynching, however, did serve to strengthen the NAACP.

Zangrando has written an important book, one that goes a long way toward explaining American race relations today. The civil rights legislation of the 1960s provided greater freedom for black people, but it has not had the effect of protecting them from mob violence. Furthermore, many white Americans appear to feel that racial justice is not an important item on the American agenda. Leaders of the NAACP felt that because lynching was one of the most brutal acts possible, widespread public support would be forthcoming once citizens and public officials were made aware of the facts. But they overestimated the humanity of both.

In preparing this book, Zangrando consulted manuscripts and public documents. In addition, he conducted interviews with many of the persons involved. He has constructed a readable document that is often too vivid in its portrayal of the gory details of lynchings.

ALPHONSO PINKNEY
Hunter College,
City University of New York

IRVIN M. MAY, JR. *Marvin Jones: The Public Life of an Agrarian Advocate*. (Centennial Series of the Association of Former Students, Texas A&M, number 8.) College Station: Texas A&M University Press. 1980. Pp. xv, 296. \$22.95.

Farm life and agricultural policy of the 1930s continue to receive attention from historians, and this biography shows the role of one overlooked figure of the era, Texas Congressman Marvin Jones, chairman of the House Agriculture Committee, 1931-40. To be sure, his importance in rural affairs did not match that of Henry Wallace, George Norris, M. L. Wilson, and other better known agrarians, but Jones was frequently a busy congressional figure by virtue of his committee post. In this study, Jones's contribution to the drafting and passage of legislation as important as the Agricultural Adjustment Acts and his administrative leadership in the War Food Administration are clearly presented. Two chapters deal with his career in the United States Court of Claims.

A product of the Texas Panhandle, Jones went to Congress in 1916 and ascended to his chairman's position in December 1931. A "man of the soil," he always sought relief for the farmers and welcomed the reduction programs set forth by M. L. Wilson and others. To some extent he questioned Roosevelt's "Hundred Days" requests dealing with non-agricultural problems and "admitted that he would not have supported many New Deal measures except in the economic emergency that gripped the nation" (p. 109). Still, he was a prairie populist, for on the subject of banks and monetary reforms he said: "The solution [to the farmers' ills] must be public ownership of banks" (p. 109). He usually preferred practical thinkers as opposed to idealists; he sought the advice of agricultural journalist Clarence Poe, for example, and avoided Jerome Frank and Lee Pressman. Like many legislators, he generally supported Roosevelt and differed mostly over details. According to May, "Jones alternated between liberal and center" (p. 100).

By 1940 he sought the security of the bench and persuaded Roosevelt to appoint him to the Court of Claims. During the war he headed the War Food Administration and handled it well, wisely selecting his lieutenants and delegating responsibility. As judge he examined the law from congressional intent rather than legalities. And he still invoked his views as a product of the Texas prairie in writing decisions.

May made extensive use of the Jones papers, plus a large number of other manuscript collections. His understanding of Jones is, therefore, deep, and the connection between Jones the prairie farmer and legislator is handled well. One reads with a sense of ease that the biographer has captured his subject. For political and agricultural historians this study must be read because it shows the importance of research on the congressional side of epoch-making legislation.

D. CLAYTON BROWN
Texas Christian University

ANTHONY J. BADGER. *Prosperity Road: The New Deal, Tobacco, and North Carolina*. (The Fred W. Morrison Series in Southern Studies.) Chapel Hill: University of North Carolina Press. 1980. Pp. xviii, 295. \$20.00.

Although much has been written on agriculture and farm policy in the 1930s, *Prosperity Road* is the first full-scale historical study of how a single farm crop fared during the New Deal years. As such, it is a welcome addition to the literature dealing with economic recovery in the depression decade.

Anthony J. Badger begins his book by describing tobacco farming in North Carolina and discussing the pitiful conditions among growers of flue-cured tobacco in 1932. He then shows how the New Deal raised tobacco prices through marketing agreements and acreage restrictions in 1933 and 1934 and how tobacco producers came to associate compulsory production and marketing controls with fair prices and a decent living. Support for the Kerr-Smith Act of 1934 reflected the demand by producers to influence prices by controlling the amount of tobacco marketed.

Between 1934 and 1940 action by the Supreme Court, changing views among tobacco farmers, and shifting political alignments combined to frustrate the development and implementation of any consistent tobacco policy. By 1940, however, marketing controls had become a permanent part of the tobacco program. Indeed, as Badger shows so well, through the careful use of political power, tobacco received special government treatment not enjoyed by other farm commodities. Just how tobacco won its favored position is an interesting and significant part of this study.

One of the most common criticisms leveled against the Agricultural Adjustment Administration has been that it extended help mainly to the larger, more prosperous farmers and actually made conditions worse for thousands of tenants and sharecroppers. While Badger found that some sharecroppers and tenants were displaced in North Carolina's tobacco regions because of acreage adjustments, he did not find any wholesale removal of these smaller, dependent producers. Tenants and sharecroppers did not always get their full share of government benefits, but all classes of tobacco farmers who continued to farm were better off under the tobacco program. The author emphasizes that the main objective of the AAA was to raise prices and increase farm income rather than to produce basic social change. Badger is to be congratulated for keeping his discussion in the historical context of the 1930s rather than making judgments based on conditions in other periods. He clearly shows that there was neither philosophical nor practical political support for changing the structure or class sys-

tem within American agriculture in order to help the rural disadvantaged.

Badger has done a thorough job of describing and analyzing the New Deal tobacco program in North Carolina. He has dealt effectively with the economic problems in that sector of American farming, and he has also added considerably to our understanding of the politics of agriculture. *Prosperity Road* is based on a wide variety of primary and secondary sources. It is a book that scholars interested in the New Deal, recent agricultural history, and farm politics will find most useful.

GILBERT C. FITE
University of Georgia

NORTH CALLAHAN. *TVA: Bridge Over Troubled Waters*. New York: A. S. Barnes or Thomas Yoseloff, London. 1980. Pp. 420. \$17.95.

Today the Tennessee Valley Authority is the largest producer of electric power in the United States and the largest consumer of coal as well. Having begun life in 1933 as a multiple-purpose river control agency, it has emerged in successive stages as a showcase of regional development under the New Deal (1930s), a major source of electricity for mobilization and atomic energy (1940s), the focus of the Dixon-Yates controversy (1950s), a symbol of both conservation and environmental degradation (1960s), and a microcosm of the international energy-ecology-cost dilemma (1970s). Throughout this eventful history the only constant has been TVA's inability to escape controversy. Whether the battles have had to do with public versus private power or bureaucrats versus snail darters, the agency has lived in the center of one storm after another.

Because of these controversies, and because of its intense interest to foreign observers, TVA has inspired an enormous literature from many disciplines. Scores of books and literally thousands of articles have praised or damned everything from TVA's accounting and personnel policies to its agricultural and flood control programs. North Callahan's new book takes note of this literature throughout the text and in a forty-two-page bibliography, which, though unannotated, is among the most useful contributions of the book.

A native of the Tennessee Valley, Callahan makes clear that his book is a labor of love but that he also recognizes the blemishes on his subject. He sets for himself the task of telling the TVA's entire story from its origins in the 1920s to about the middle 1970s. Chapters entitled "Power and Controversy," "TVA at War," "On the Personnel Side," and "Into the Atom Age" measure the reach of the author's ambitions.

Unfortunately, Callahan's approach to his

lengthy and complex story is informal to the point of chattiness. A substantial proportion of the book is made up of direct quotations (one exceeding three pages in length) from journalists, public figures, other scholars, TVA reporters, and valley residents. The author, choosing not to employ footnotes, in several places loosely paraphrases other scholars. The book is rambling and disjointed, lacking the interpretive structure, careful framing of important questions, and selectivity of material that scholarship demands of a work that attempts to cover forty years in the life of an organization as large and complex as the TVA.

In the great mass of TVA literature, Callahan's book does have the virtue of moving beyond the 1930s and following the agency into new times and new problems. In this it is a useful and easy introduction to unmapped territory, accompanied by a wealth of anecdotal material and accomplished with a certain charm.

THOMAS K. MCCRAW
Harvard Business School

DAVID L. PORTER. *Congress and the Waning of the New Deal*. (National University Publications Series in Political Science.) Port Washington, N.Y.: Kennikat Press. 1980. Pp. xiv, 169. \$13.50.

This book is basically a study of six legislative battles in the last of the peacetime Congresses of the New Deal, 1938-40. David L. Porter has selected the disputes over the monetary program, transportation regulation, reciprocal trade, work relief, executive reorganization, and political activities of federal employees. Individual chapters deal with each of these topics and the debates over the amendments to the various pieces of legislation. Using a variety of sources from congressional correspondence and the published record to roll call data, the author provides background for his basic thesis: that the period is an important but neglected one in which Congress reasserted its control over national policy. That reassertion of control, rather than a clearcut ideological or partisan split, is the central characteristic of a third phase of the New Deal that follows the so-called First and Second New Deals. Given the fact that most interpretations settle for a view of the period as one of relative stagnation or of suspenseful waiting for the war to start in Europe or for Americans to decide to enter it, the attempt to focus on political policy and practice with regard to domestic issues could be significant. The idea is an important one, and more historians ought to try to pick it up.

Porter's study is useful but so flawed that it raises more questions than it answers. The rationale for selecting these particular pieces of legislation and

ignoring others is unclear. As is so often the case with dissertations, the principle of selection appears to be simply the fact that others have not dealt with these specific materials extensively or have not done it well enough. The search for a new intellectual framework would have been important, even if it had been necessary to include familiar materials to be viewed from a new and potentially exciting perspective. The narrowness of focus makes it difficult to relate the details of floor squabbles to presidential efforts at policy making, let alone attitudes of the public supposedly being represented in Congress. There is no larger political scene, little sense of generalization or direction, and little attempt to define the significance of what the author feels, correctly I believe, is a significant sequence of events. This book will be helpful for anyone who wants to take another look at this important period of the New Deal; but the work of reconceptualizing the late or "waning" New Deal is still to be done.

BARRY D. KARL
University of Chicago

GERALD T. WHITE. *Billions for Defense: Government Financing by the Defense Plant Corporation during World War II*. University: University of Alabama Press. 1980. Pp. viii, 194. \$16.50.

This compact monograph may have the appearance of esoterica, but readers will find it a timely book on an important public policy issue. When markets are deemed incapable of producing certain outcomes desired by society, how may governments induce them? Today, as we ponder ways to revive center cities, refloat Chrysler, extract oil from rock or chicken manure, make the railroads run, and so on, there are intense arguments over policy options. In our climate of skepticism about government competence, the tide runs toward tax incentives or direct subsidies, rather than forms of public ownership. Gerald T. White has given us the story of a successful government effort to induce an outcome outside the market system—the Defense Plant Corporation (DPC) of World War II, which added a crucial 30 percent to America's defense production capacity in the emergency of 1941–45.

The story is absorbing. A subsidiary of the Reconstruction Finance Corporation (RFC), the DPC embodied the direct investment strategy (the tax incentive strategy was used to finance about one-fourth of defense facilities, as against one-third for DPC's government ownership approach). The usual pattern was for DPC to finance the construction of a facility, then lease it to a private corporation for the duration of the war.

White's story necessarily proceeds through some dense detail, but so clearly drawn are the issues and

personalities that it must stand as the most interesting bureaucratic history I have perused this year. Government bureaucrats, now assumed to be invariably stupid and unimaginative, used DPC's \$7 billion to build the Big Inch and other pipelines, launch the synthetic rubber industry, break Alcoa's monopoly on aluminum, fund enormous expansion in the aircraft, chemical, and steel industries, most of this in the teeth of resistance from private "entrepreneurs." The government was then the risk taker, businessmen hanging back from new technologies or plant expansion out of dislike for the contract process (which might pierce the veil of corporate secrecy), fear of excess capacity, lack of enthusiasm for a war against fascism, or temperamental conservatism. RFC's head, Jesse Jones, is a central figure, shrewd in the details of contract negotiation but lacking in imagination and instinctively siding with large industries when New Dealers like General Counsel Clifford Durr sought to use DPC influence to promote small business. In those days, there was considerable scope for bureaucratic creativity, and in addition to funding large plants the DPC bought tugs, barges, buses, railroad cars, and machine tools, deploying them where bottlenecks occurred, and even spent \$27 million on public housing. It must have been, for four years, the country's most interesting conglomerate.

The largest issues raised by the DPC experience arose when it was to be liquidated. Would the public recapture its investment through orderly sale at highest market value, or would surplus property be generously disposed of to corporate operators in a massive hidden subsidy? Would the idea of continued public ownership have any influence as facilities were demobilized? Would the disposal process be guided by antimonopoly sentiment, then encouraged by the Truman committee, Secretary Ickes, and others, or would large firms be given an inside track on acquisition of surplus plants? It was a moment of considerable social opportunity, for the stakes were enormous. DPC disposed of \$16 billion in plant and equipment, since time had raised the value of its investment, and this amounted to probably 15 percent of the nation's industrial capacity and virtually 90 percent of the plant in the rubber, aircraft, and magnesium industries. While inadequate records hampered White's search for a clear view of the outcome, it appears that New Deal impulses were weak and the disposal process pursued no larger social goals. "The range of meaningful debate was not great, and the outcomes were distinctly conservative," White concludes (p. 90), as he writes a valuable chapter of the war effort and the end of the era of Rooseveltian liberalism.

OTIS L. GRAHAM, JR.
University of North Carolina,
Chapel Hill

ROBERT W. ROSS. *So It Was True: The American Protestant Press and the Nazi Persecution of the Jews*. Minneapolis: University of Minnesota Press. 1980. Pp. xvii, 374. Cloth \$20.00, paper \$9.95.

The literature on the Holocaust is long and growing. For almost two decades after World War II ended the accounts stressed the inhumanity of Hitler and the Nazis who annihilated six million European Jews. Since the publication of Rolf Hochhuth's play, *The Deputy* (1964), in which the author argued that Pope Pius XII should have made a greater effort to thwart the Nazis' "Final Solution," others have become the prime targets in Holocaust literature. From the late 1960s through the early 1970s Arthur Morse (*While Six Million Died*), David Wyman (*Paper Walls*), Henry Feingold (*The Politics of Rescue*), and Saul Friedman (*No Haven For the Oppressed*) exposed the callousness and indifference of American officials who refused to make any significant attempt to save Europe's Jews. In 1979 Bernard Wasserstein (*Britain and the Jews of Europe*) detailed the inhumanity of British government members who refused to open the gates of Palestine and who, in addition, actively intervened with other nations to prevent Jews from escaping the Continent. Now we have another book, *So It Was True*, that reproaches the American Protestant press and condemns the editors of the religious newspapers during the years 1933-45 for silently standing by while Jews were "made victims of unchristian policies perpetrated by nominal Christians" (p. 263).

It is a powerful study that makes quite clear that American Protestants received "extensive, continuous, and sometimes comprehensive" (p. 259) coverage of Nazi policies towards Jews during the twelve years that Hitler remained in power and asks as well: "Why did not the Christians speak out? Demand assistance for the Jews? Pressure the American government to act? Why did not the American Protestants themselves act in concerted, massive, total Christian coalitions of protest and assistance" (p. 259)? Indeed, why did they not do these things?

Robert W. Ross surveyed fifty-seven Christian newspapers in the United States for the period of his study. His book is replete with examples of how extensive the news coverage, paid advertisements, and occasionally editorials, were of Hitler's Jewish policies. In general, however, the papers rarely commented on the brutality and inhumanity of what they recorded and even questioned sometimes the "atrocious stories" that they considered exaggerated. But there is such an abundance of rich detail in this book that few readers would question its conclusions. The most devastating one, perhaps, is that "thirty-three years . . . after the Holocaust, the 'gen-

eral stillness' of American Protestantism still hangs like a pall over all Holocaust studies" (p. xiii).

This is a fine study. My only criticism is that a fuller analysis of the American context would have shown that the Protestants were not alone in failing "to act in . . . massive . . . coalitions of protest and assistance."

LEONARD DINNERSTEIN
University of Arizona

KENNETH RAY BAIN. *The March to Zion: United States Policy and the Founding of Israel*. College Station: Texas A&M University Press. 1979. Pp. xviii, 235. \$15.95.

The Israel-Palestinian conflict is a flashpoint in the international crisis, and Zionism, the ideology that gave birth to Israel, earns almost monthly a disapproving resolution in the UN. Thirty-three years, the time elapsed since that same agency helped establish the state, may be too brief a period to compose a value-free diplomatic history. Kenneth Ray Bain's book, the latest in a series of four to have appeared on the same topic in a short period, bears testimony to the fact that the conflict between Arabs and Jews has found yet another front in the field of American diplomatic history.

Bain's work is openly sympathetic to the Palestinian cause, one largely ignored by decision makers in 1948. What grieves him is that American diplomacy was instrumental in solving one injustice against the Jewish people at the expense of a "remote and defenseless" people, the Palestinians. A haven was found for the survivors of the Holocaust, who might have been absorbed elsewhere, by displacing the Palestinians. The notion that these immigrants, especially when accompanied by capital supplied by the American Jewish community, might have served as stimulants to the economy and increased its absorptive capacity for both Jews and Arabs is not examined. That is what actually did happen.

Bain sympathizes with the victims of the Holocaust but has little understanding of the valence of an event that slaughtered one-third of the Jewish people—all for lack of a haven. He seems unaware that time no more weakened the longing and claim of Jews for the Holy Land than it does for Palestinians today. By the late thirties, that overwhelming Zionist consensus, or at least a unique Brandeisian form of it, fashioned the thinking of American Jewry. Bain falls into the same error as did some factions in the State Department at the time. Lessing Rosenwald and the American Council for Judaism did not represent a countervailing opinion among American Jews. In three centuries of Ameri-

can Jewish history no organization came closer to a Jewish version of excommunication than did the Council.

A fairly new contribution is made in two areas. The author shows how the public image of Arabs and Jews, the former pictured as "lazy natives" and the latter as "modern pioneers," made Truman's recognition of Israel a consensual decision rather than one taken because of the machinations of Zionist agents working against the American national interest. The second is Bain's instructive examination of the diplomacy of oil. A standard notion among "Zionist scholars" that behind the State Department's pro-Arabism was antisemitism is found wanting. Bain points out that one department faction actually favored the creation of a truncated state as part of a petroleum diplomacy, sensing that a state imposed by the UN would be better than years of uncertainty. By 1948, the department was more obsessed with the threat of Communism than it was imbued with antisemitism. What bothered some was the sense that the Jewish displaced persons entering Palestine had been infected with the Communist virus. Many undoubtedly believed that Jews were especially prone to such disorders.

In breadth of research and use of secondary material this work falls slightly below prior works in this area. But in terms of the quality of the narrative, making words obey and convey, it is very satisfactory.

HENRY L. FEINGOLD
Baruch College,
City University of New York

THOMAS A. BAILEY. *The Marshall Plan Summer: An Eyewitness Report on Europe and the Russians in 1947*. Stanford, Calif.: Hoover Institution Press. 1977. Pp. viii, 246. \$10.95.

Preparatory to teaching at the National War College, Thomas A. Bailey was sent on a tour of Europe in the summer of 1947. This book is the story of his trip at that time.

Bailey means the essay as a document—his diary of the period—and it does seem to contain much raw material composed at the time. But its worth as a primary source is compromised because the narrative was reconstructed some thirty years later; it is informed by Bailey's later ruminations on this period of American history and on papers on foreign affairs only recently released. As Bailey notes, the views are those of his *America Faces Russia* (1950). The book's value as a memoir of 1947 is further diluted by Bailey's wide-ranging personal reminis-

cences of other periods of his life and his reflections on other episodes in American diplomatic history.

BRUCE KUKLICK
University of Pennsylvania

LARRY CEPLAIR AND STEVEN ENGLUND. *The Inquisition in Hollywood: Politics in the Film Community, 1930-1960*. Garden City, N.Y.: Anchor Press/Doubleday. 1980. Pp. xiv, 536. \$17.50.

With the recognition of its enormous persuasive powers, the visual media have been the intense focus of political anxiety, particularly on the part of conservative elements in the United States who have feared the encroachment of the left in media institutions. This thorough study of the politics of the cinema community from the depression through the post-World War II decades concentrates on the complex political struggles between three organizations of media persuasion—the Screen Writers Guild, the Movie Moguls, and the Dies and House Committee on Un-American Activities—plus a number of related organizations. It was a struggle that resulted in the purification of the film industry from radical influence.

This is a work of considerable dimension that unintentionally is torn between opposite sentiments regarding the left movement in Hollywood: between homage and criticism, compassion and distance, innocence and sophistication. It manages to balance these tensions through detail and a sweep that fuses biography and institutional activities and thereby fills an important section of the history of political repression in the twentieth century. This work is also excellent social history when it takes us into the committee rooms and living rooms of the protagonists, particularly the radicals in Hollywood as they futilely wrestled with the problem of reconciling movement demands with creative impulses. The now symbolic groups of the period, the Hollywood Ten and the Blacklisted Hundred, are deftly traced as they make their way into jails or into exile here and abroad.

Questionable, however, are Larry Ceplair and Steven Englund's contention that the anti-communist liberals, as the crucial swing group, not only hindered but prevented the formation of a liberal-radical alliance during the late forties and early fifties: "as the historians of this era, [they] gave final legitimacy to the anti-Communist crusade" (p. 205). Even if liberals had managed to submerge their anti-Soviet attitudes, it is doubtful that the conservative reaction could have been stemmed. It is certainly ironic that the film industry was done in by a group of Congressional politicians who cleverly utilized the techniques that Hollywood had devel-

oped. Moreover, the statement that "it was not especially easy to link communism with un-Americanism and thus make it *the* issue in the country" (p. 202, authors' italics) misses the historical mark and the authors' own salient understanding of the Red Scare. That we have already experienced three repressive movements in the span of forty years speaks to an extraordinary political pattern in American culture. Given a similar climate of anxiety and threat, the possibility of another such reaction clearly is to be anticipated.

JOSEPH BOSKIN
Boston University

THOMAS BECNEL. *Labor, Church, and the Sugar Establishment: Louisiana, 1887-1976*. Baton Rouge: Louisiana State University Press. 1980. Pp. xiii, 222. \$20.00.

In this slender volume, Thomas Becnel adds yet another chapter to the already depressing story of the failure of Southern agricultural laborers to achieve even an approximation of economic justice. Although the work focuses on the efforts of the National Agricultural Workers Union, led by H. L. Mitchell, to organize cane cutters in the sugar fields of Louisiana in the 1940s and 1950s, the events chronicled could have occurred during the efforts of the Knights of Labor to organize agrarian day laborers. They could have been taken from the history of the Industrial Workers of the World among Southern loggers or the Southern Tenant Farmers Union among the region's farmers in the 1930s.

Encouraged by NAWU organizers to join the union and force cane growers to pay decent wages, cane workers struck Louisiana's major cane farms in the fall of 1953. Poorly organized, woefully underfinanced, and representing only a small fraction of the total number employed in the cane fields, the NAWU strikers were doomed to failure. Allied against them were the powerful American Sugar Cane League and its political allies, including Senator Allen Ellender, state and federal agencies more attuned to the needs of planters than laborers, and the local and state press. The defeat of the 1953 strike effectively crushed the NAWU and fueled a political backlash that resulted in the passage of Louisiana's first general right-to-work law in 1954. Although repealed two years later, the general law was immediately replaced by a right-to-work law that applied to agricultural laborers only. To insure the repeal of the general right-to-work statute the Louisiana AFL-CIO supported the agricultural right-to-work bill, an action endorsed by the national organization. Passage of the agricultural right-to-work bill, which was upheld by both state

and federal courts, insured the failure of all future efforts to organize Louisiana's farm laborers.

What is unique about this volume is the emphasis placed upon the role of the Catholic church in the labor disputes between cane growers and field laborers. Becnel demonstrates beyond question that the church, under the leadership of the archbishop of New Orleans, Joseph Francis Rummel, was thoroughly involved in and supportive of NAWU organizational efforts. Priests wrote union newsletters, led organization meetings, ferried organizers, raised finances, indeed, provided much of the NAWU's organizational manpower. The archbishop personally urged cane growers to recognize the right of their agricultural workers to organize, to no avail. Although Catholic southern Louisiana presents a special case (the uniqueness of which Becnel would have done well to analyze further), the relationship between the church and the NAWU suggests that Southern churchmen were not oblivious to the economic injustices within the society in which they served.

Like Donald Grubbs, whose *Cry From the Cotton* this work complements beautifully, Becnel is highly critical of the agricultural agencies of both state and federal government. Even Louisiana State University, which published the volume, comes under rather heavy criticism for allying with the planters against their labor force. Becnel forcefully argues that the entire federal bureaucracy ignored the plight of the agricultural laborer. The Department of Agriculture, the Department of Labor, the FBI, even the War Department, which furnished German prisoners of war as cane cutters, are all indicted.

Thoroughly researched, well organized, and clearly written, this work provides fresh insight into the relationship of the church and organized labor in the South while providing labor historians with yet another detailed account of failed union efforts among the South's rural poor.

MELTON A. MCCLAURIN
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RICHARD H. K. VIETOR. *Environmental Politics and the Coal Coalition*. (Environmental History Series, number 2). College Station: Texas A&M University Press. 1980. Pp. xiv, 285. \$19.50.

This is a study of the twenty-year struggle between coal interests and environmentalists over strip mining and air pollution. On this level, it is superb. Richard H. K. Vietor convincingly demonstrates the existence of a "coal coalition"—the complex of advisory councils, trade associations, and lobbies

created by coal-related companies in the late 1950s and 1960s to defend their interests in state and national politics. The part of Vietor that is political scientist is careful to tell us how this new kind of coalition (in which railroads, banks, and other companies with only a secondary interest in coal cooperate for political ends) intersects with existing theories of pluralism. The remainder of the book is legislative and administrative history. Vietor illustrates how the coalition sought to keep the regulation of strip mining and air pollution in the states, where it could more easily dominate the politics of implementation, and how environmentalists sought to remove these issues to national arenas where the coalition would be less likely to have its way (there is a brief treatment here of Gabriel Kolko's opposing view of federalism). By the mid-1970s the state-federal issue had been superseded by the polar claims of environmentalists who wished to limit economic growth and of those, including the coal coalition, who sought unlimited energy development in the wake of the 1973 Arab oil embargo. These and other themes ripple through a rich and lucid narrative based on unusually thorough research in materials recently generated by government agencies, notably the Commerce Department.

On another level, however, the book is marked by a subtle ambivalence about the issues it treats. On the one hand, Vietor describes the coal coalition as a kind of juggernaut of organization, vital information, and "expertise" that threatens our democratic processes. The environmentalists, in contrast, are "amateurs," overwhelmed by industry's "control and use of data" (p. 9). That anything good could come of this mismatch would seem unlikely. And yet Vietor's tentative judgment of the 1977 strip mining and air pollution amendments is that they reflect the no-growth philosophy of the environmental left.

Like many of the Vietnam-Watergate generation, Vietor is skeptical of democratic politics, especially when those politics revolve so much around information accessible only to big business. But Vietor shares with the new crop of economic realists born in the energy crisis the hunch that the coal coalition, in emphasizing technological feasibility, cost, and general practicability, has reason on its side. Environmentalists, disinterested in economic implications and committed to policies like "no significant deterioration" (of existing air quality), are short-sighted and unreasonable. With one foot in the camp of the New Left, and the other in that of the economic New Right, Vietor has written a book that for all its clarity of presentation is very much a product of rapidly changing national priorities.

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HERBERT S. PARMET. *Jack: The Struggles of John F. Kennedy*. New York: Dial Press. 1980. Pp. xvii, 586. \$14.95.

With a few notable exceptions, Herbert S. Parmet's useful biography of John F. Kennedy's life from his birth until his formal announcement in early 1960 that he would run for president covers familiar ground. Parmet does provide interesting new information about Kennedy's various physical ailments and his energetic sex life before (but not after) his marriage. Although the latter may conceivably help to sell the book, prurient readers will likely be disappointed. About Jack's love affairs, Parmet tells who, when, and sometimes where; but he provides no lurid details. As for JFK's health problems, Parmet argues firmly that Jack had Addison's disease, something that Kennedy consistently denied. Even if he did not, he had more than his share of recurring, serious ailments, as this book clearly documents. Indeed, JFK's physical problems apparently constituted a significant portion of the "struggles" referred to in the book's rather cryptic subtitle.

Another major "struggle" for Kennedy as he matured was his effort to moderate the influence of his forceful and controversial father, Joseph P. Kennedy, Sr. For this reviewer, what lingered on most vividly about the book long after it was read was the way the senior Kennedy dominated and manipulated the entire Kennedy family. An incredible wheeler-dealer, he used his wealth and power in countless ways to further his children's careers. He demanded and usually got his own way, both from family members and from others. As Parmet explains, JFK was only very gradually able to manifest his independence from his father.

Jack is particularly valuable for its description of JFK's years in the House and Senate, not because it provides any startling new insights but rather because the author captures so effectively Kennedy's shifting political attitudes. As a young congressman and even in his early years as a senator, he was not in the liberal wing of the Democratic party. Parmet notes JFK's friendship with Richard Nixon when they were relative newcomers to Congress and probes in some detail Kennedy's congenial relationship with Joe McCarthy, episodes Kennedy supporters would undoubtedly prefer to forget. The point is that JFK acquired his liberal credentials by fits and starts and not until the mid to late 1950s. Even then, skeptics suspected that JFK's emergence as a liberal champion was prompted primarily by political considerations. Parmet, it should be noted, is more generous in his appraisal of Kennedy's motives.

The book is not particularly gracefully written, but this is a relatively minor complaint when compared with its other fine qualities. Based heavily on

papers and oral histories available at the Kennedy library, it is carefully researched and richly detailed. Above all, the book is distinguished by Parmet's even-handed and balanced treatment of JFK and his family. As a result, *Jack* ranks as the best account of Kennedy's pre-presidential career published thus far. A succeeding volume, in which Parmet will examine the Kennedy presidency, should be worth waiting for.

JIM F. HEATH
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MICHAEL W. MILES. *The Odyssey of the American Right*. New York: Oxford University Press. 1980. Pp. x, 371. \$19.95.

In this well-written book, Michael W. Miles discusses the development of right-wing Republicanism since the New Deal. He holds that the right, launching a "counter-offensive" delayed since 1938, endangered the dominance of the New Deal majority in the years 1948-54 and re-emerged with energy after 1964. Miles asserts that Watergate set the right back once again, but he does not take his study beyond 1974.

The strongest section of *The Odyssey of the American Right* is the middle (chaps. 5-11), which focuses on the 1940s and 1950s. Miles effectively discusses foreign policy differences among Republicans in the period, setting up three categories: "old nationalists" (isolationists), "new nationalists" (strong anti-communists), and "Eastern internationalists." He offers no revisions of existing views but provides an excellent explanation of how various groups of Republicans came to the policies they did. Miles might have marshaled less impressionistic evidence to support his categorizations, but his discussion of the debate over China policy, differences over McCarthyism, and development of the "liberation" doctrine is highly informative.

On the other hand, the author's treatment of events after 1960 is weak. He explains the "New Right" by cursorily surveying its demographic sources (blue-collar Catholics, the New West, and the New South) and the Goldwater and Nixon electoral strategies. These chapters are marred by the author's neglect of published voting studies and his failure to consider the range of viewpoints represented by congressional Republicans in the period. The book inexplicably ends with the Watergate episode, rather than presenting general conclusions concerning the right in the later 1970s.

Both the strengths and the weaknesses of *The Odyssey of the American Right* derive from Miles's emphasis on ideological continuities. Persuasive on some points, he is simplistic on others—as in positing a straight-line connection between nineteenth-

century "true liberalism" and "true Republicanism" of the 1930s (p. 18) or in arguing that "new nationalist" China policy after World War II reflected "traditional" Republican views regarding Asia (p. 118), rather than Cold War political necessities. By overemphasizing continuity of viewpoint, Miles ignores the dynamic nature of the American right, which has shaped its views as frequently in response to the actions of its rivals as out of fidelity to principle.

No topic is more relevant as the 1980s begin than the transformation and resurgence of the American right. Miles's book, although disappointing on the most recent period, offers interesting and useful observations on a most important subject.

GARY W. REICHARD
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WARREN I. COHEN. *Dean Rusk*. (American Secretaries of State and Their Diplomacy, number 19.) Totowa, N.J.: Cooper Square. 1980. Pp. xii, 375.

This is a highly readable and able study of the rise of Dean Rusk from poverty to high intellectual achievement and distinguished service in both the War and State Departments. It is a brilliant condensation. Nonetheless, this analysis of Rusk's considerable contribution to the solution of our many foreign policy problems since World War II rarely offers an adequate explanation of the major problems themselves. One major issue, more than capitalism versus communism, has been and continues to be slavery versus freedom. The author fails to convey any sensitive awareness of and outrage at the repression by the Soviet Union and all totalitarian systems. It was a keen sense of this awesome evil that moved our leaders after World War II to follow a policy of containment. For example, the author faults our role in Vietnam and correctly calls Diem a rotten dictator but fails to condemn the Communists for their extreme repression of all Vietnamese.

He explains at great length the many debates within the government over the war in Vietnam, delights in distinguishing Rusk's universalist outlook, Acheson's Atlanticist view, and Kennan's realism, without giving us even a brief glimpse of what Dean Rusk, Lyndon Johnson, and millions of Americans felt was the real issue and the real danger. Many believed very deeply that communism, like Nazism, was a most inhumane force seeking to impose itself upon all mankind. Further, Cohen writes that Eden and Rusk "failed to appreciate the extent to which a national liberation movement differs from the aggression of a great power" (p. 327). This ignores that the Soviet Union advanced its power through "national liberation movements,"

which were in truth national enslavement movements.

The author describes the efforts of Kennedy and Johnson to limit both our overseas commitments and the Vietnam War. He traces our role in the United Nations, the continuing Arab-Israeli dispute, the Bay of Pigs and missile crises, the Belgian Congo conflict, and our intervention in Santo Domingo.

In his eight years as secretary of state, Rusk's greatest troubles were with Vietnam and the Soviet Union. The author shows impatience with Rusk's defense of our war in Vietnam, especially when he feels Rusk tended too often to sacrifice his own convictions for the sake of the Kennedy-Johnson team. After describing our decision to abandon Diem's "rotten dictatorship," Cohen writes: "Heaven, now located on Pennsylvania Avenue, had withdrawn its Mandate from the Diem dynasty" (p. 190). Such impatience was highly justified. There was nothing immoral about the desire to contain dictatorial communism in Vietnam and elsewhere, but there was something very wrong in trying to do it by any support of such a bloody dictatorship. Here and elsewhere we have been anticommunist when we should have been profreedom.

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WALLACE J. THIES. *When Governments Collide: Coercion and Diplomacy in the Vietnam Conflict, 1964-1968*. Berkeley and Los Angeles: University of California Press. 1980. Pp. xix, 446. \$20.00.

The Johnson administration's strategy of bombing North Vietnam in order to coerce it into ending its support of the Southern insurgency, Wallace J. Thies argues, rested on three main assumptions: (1) that a program of gradually rising pressures would convince the government in Hanoi that the cost of continuing its involvement in the South was too high; (2) that the administration would be able to "fine-tune" its actions so as to orchestrate words and deeds, negotiations and military force; and (3) that the use of force would be controllable, that is, that the bombing could be turned on and off as well as up or down at will. Why this strategy failed is the subject of this thoughtful study.

The author, a member of the political science department of the University of California, Berkeley, rejects the argument that a hard-blow approach would have succeeded where a gradual escalation of pressure failed. Instead he suggests, first, that the Johnson administration did not understand its opponent in Hanoi. It ignored the deep commitment of the Communist leadership to the goal of com-

pleting the revolution in South Vietnam, which these men had pursued for virtually all of their adult lives and on which careers and reputations had been staked. Secondly, the American government's approach slighted the bureaucratic difficulties of developing a coherent strategy that would send a clear signal to Hanoi. Senior officials assumed that their own decisions would be implemented accurately and promptly and that the "message" they intended to transmit would be the same as the "message" read into their actions by their opponents. These assumptions proved incorrect; they resulted in misperceptions and distorted communications, which contributed to the failure of negotiations. Finally, President Johnson's policy of staking out the middle ground between doves and hawks proved incompatible with the controlled use of force. The preoccupation with achieving a policy of consensus introduced rigidity and greatly complicated the task of striking a deal with Hanoi.

Thies develops these conclusions from a careful review of U.S. and North Vietnamese policy, and these chapters of the book may be of most interest to historians. The author skillfully uses the diplomatic volumes of the *Pentagon Papers*, not leaked to the press, as well as captured documents and interviews with prominent Communist defectors. Running counter to the conventional wisdom is the finding that at several crucial points it was a decision of Hanoi to escalate that preceded a similar decision in Washington. Thus the decision to launch the armed struggle in the South came nearly two years before the major increase of the American effort under President Kennedy. The commitment of regular North Vietnamese units to the war in the South preceded by several months President Johnson's decision to begin the air war against North Vietnam.

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MELVIN SMALL. *Was War Necessary? National Security and U.S. Entry Into War*. (Sage Library of Social Research, number 105.) Beverly Hills, Calif: Sage Publications. 1980. Pp. 311. Cloth \$18.00, paper \$8.95.

There is a certain kind of historical work in these pages that is particularly vulnerable to criticism. It usually dispenses with an index as well as footnotes and appeals directly to a popular audience presumably unfamiliar with the details of American history. The style is colloquial and the thesis easy to digest. Melvin Small's book, with its rhetorical title, seems calculated to raise scholars' hackles. It fits all the objections listed above.

It would be a pity if his "breaking several canons

of scholarship" (p. 25) should put off potential readers. Small has a good deal to say, and he says it well. He examines six major wars from the War of 1812 to the Korean War in an extended effort to learn if national security was involved in any or all of the conflicts. His definition of national security is clear; it covers primarily the threat of "direct military attack on our own soil or on the soil of a near neighbor whose conquest would leave us in a dangerously weakened position" (p. 19). To this he adds consideration of economic threat and attack on the nation's honor and prestige.

Small offers no surprises; he warns the reader explicitly in the preface and introduction that no war between 1812 and 1950 was justified by a sense of national security and that his own judgments are implicitly influenced by the American experience in Vietnam. References to Vietnam appear in chapters on every war except the War of 1812. Making use of materials he cites in the useful bibliographies appended to each chapter, he presents a judicious as well as readable account of the diplomatic background of the wars. Occasionally, generalizations are simplistic or misleading, particularly in his characterizations of Jefferson and Madison, but his attempts to reach balanced conclusions are generally successful. He makes pertinent connections between Colonel House's and Kissinger's functions as well as between Polk's apparent chance for president in 1844 and Senator Dole's in 1980.

Still, the book must be judged by how convincing are his tests of national security. Although Small has been a leader in efforts to educate his colleagues in the application of social science techniques to diplomatic history, these chapters offer little insight into the problems that he set for himself in this volume. An appendix dealing with the correlations of American wars tells him only that most hostilities were initiated in the spring. There is no statistical evidence, simply his definition, to support his judgment. What is missing in his evaluations is the essentially unmeasurable potential results to the nation if wars had not been waged at the times they were. He alludes only vaguely to the society America might have become in the wake of an Axis victory. There is an implication for national security in his observation that "if we went to war over remote Korea, we would surely go to war to save European democracy" (p. 292). National security deserves a broader definition than the one he has given us.

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CANADA

HUGH JOHNSTON. *The Voyage of the Komagata Maru: The Sikh Challenge to Canada's Colour Bar*. New York: Oxford University Press. 1979. Pp. 162. \$12.95.

The Komagata Maru incident was one of the more dramatic episodes in Canadian immigration history. In 1914, 376 Sikh peasants on board a chartered Japanese ship challenged Canadian immigration laws that then barred all East Indians from entering the nation. For two months the crowded ship lay at anchor in the Vancouver harbor while immigration officials and the courts heard the Sikhs' appeal, which ultimately proved fruitless. Defying federal deportation orders and repulsing one boarding attempt by police and government officials, the would-be immigrants were finally forced to leave, escorted from Canadian waters by an over-age ex-British cruiser—at that time Canada's entire Pacific fleet. Because of their suspected links with the Indian nationalist movement, the challengers were also regarded with distrust by the Indian government, then extremely sensitive to threats against the raj. Upon disembarking near Calcutta the returning Sikhs met a group of suspicious, obdurate officials and, in the conflict that ensued, troops opened fire, killing eighteen of their number. These events quickly passed into the mythology of the Indian nationalist movement, the passengers accorded martyr status, their leader, Gurdit Singh, invested with the honor of a nationalist revolutionary hero.

This slender book tells the story of the incident, from the first Canadian attempts to bar the door to East Indians to the influence of these events upon the Indian nationalist movement. It is the fullest account of the episode published to date and the only one that links its Canadian and Indian phases. The particular strength of the study lies in its description of the events of the conflict and its various antagonists: Sikh peasants and revolutionary Indian exiles, Canadian politicians and immigration officers, British Indian police spies and government officials, East Indian dissidents and nationalist leaders.

The limitations of the book are chiefly those of narrative history. While it sketches character and circumstance with a fine sense of the dramatic, the study also abstracts these events from the Canadian and Indian contexts in which they were set. In British Columbia, Asian immigration had been a source of deep public concern for more than half a century when the Sikhs set sail for Vancouver. Vivid though they were, these events were only a minor incident in a long history of strained race relations in western Canada and, for that matter, western America as well. Then, too, the voyage and its aftermath were lesser episodes in the history of modern Indian nationalism. Because of its genre, this book fails to analyze the larger relationship between the incidents that it recounts and the social and political movements of which they formed but a small part. In so doing, it does not fully explain what it so clearly and effectively describes.

W. PETER WARD
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WILLIAM L. MARR and DONALD G. PATERSON. *Canada: An Economic History*. Toronto: Macmillan of Canada; distributed by New York University Press, New York. 1980. Pp. xx, 539. \$18.95.

The study of economic development in Canada has been linked with nation building in a tradition of "political economy" going back to John Locke. A generation ago Canadian economists adapted economic theory to Canadian peculiarities through the staples thesis, an argument explaining the pattern of Canadian economic, social, and political changes during successive stages of resource exploitation. The staples paradigm, a variant of export-led growth, has enjoyed a stranglehold on the Canadian academic imagination, only recently coming under attack (or being ignored) by younger scholars. The two economists, William L. Marr and Donald G. Paterson, who have written this book rely heavily upon the staples approach to structure their data. But instead of discussing Canadian economic development around various staples, as H. G. J. Aitken and W. T. Easterbrook did in 1956, they emphasize instead how changing resource endowments affected production functions, the national income, and sectoral shifts in output. The book purposefully slights seventeenth- and eighteenth-century economic changes in favor of nineteenth- and twentieth-century development strategies.

For this reason the book seems more suited for collateral reading in economics courses than for history students, and its organization, content, and emphases reinforce this impression. The authors willfully abandon chronology in favor of a topical arrangement designed to expose "the forces of economic change and their consequences" (p. xvii); they also incorporate many formulas and graphs normally found only in economics texts, assuming that their readers already have a familiarity with both the basic principles of economics and the broad outlines of Canadian history. Their chapters generally sweep back and forth over three centuries of Canadian capital formation, transportation, agriculture, and population change. Admittedly the latter chapters focus largely upon twentieth-century concerns, and the authors are probably correct in assuming that the forces of economic change, as adumbrated by staples theorists, are indeed more clearly seen through such an arrangement.

Whether they are more adequately explained is open to question. By abstracting these forces from their broader historical contexts, the roles of individuals and groups are ignored. Thus Sir John A. Macdonald, presumably a major influence on the nineteenth-century Canadian economy, is mentioned just three times in this book, twice in passing. The chapter on labor discusses only labor markets, not flesh-and-blood workers who are reduced here

to passive commodities molded entirely by community attitudes or demands for their skills and muscles. The authors falsely conclude that both the frequency and length of strikes in Canada are better explained by the monopsony power of Canadian employers rather than by the heightened class consciousness of workers. The authors' treatment of economic retardation in the Maritimes (pp. 425-38) fails to cite entrepreneurial weaknesses. In short, their thematic arrangement removes discrete events from wider historical contexts, and their conclusions exaggerate the role of impersonal forces over the decisions of individuals and groups.

Historians will consult this book mostly for its synopses of theories and policies. It is a useful compendium of historical data on the Canadian economy. Yet they will still rely on Aitken and Easterbrook's *Canadian Economic History* because its arrangement and conclusions better reflect the canons of historical explanation.

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LATIN AMERICA

CIRO FLAMARION S. CARDOSO and HÉCTOR PÉREZ BRIGNOLI. *Historia económica de América Latina*. Volume 1, *Sistemas agrarios e historia colonial*; volume 2, *Economías de exportación y desarrollo capitalista*. Barcelona: Editorial Crítica. 1979. Pp. 232; 213.

Historia económica de América Latina by Ciro Flamarion S. Cardoso and Héctor Pérez Brignoli is not, strictly speaking, an economic history of Latin America. The authors state that theirs is not a manual that summarizes the existing knowledge of Latin American economic history. Instead, the book is designed to "aid in the construction of hypotheses, the definition of problems and priorities of research." The authors believe that interpretations of Latin American economic history as a variation of European historical stages and as dependent on advanced capitalism represent dead ends in historical interpretation. They therefore place more emphasis on agriculture than on commerce.

In volume 1, *Sistemas agrarios e historia colonial*, the first ninety pages are devoted to a general analysis of the relationship between agriculture and economic history. The next sixty pages provide a useful summary of the historical and anthropological literature of agrarian systems of Europe, pre-Columbian America, and precolonial Africa as the elements that determined colonial Latin American agrarian practice and economic development. The final seventy-five pages describe the sixteenth through eighteenth centuries, focusing on mines, haciendas, and

slavery. The authors provide a summary of the ideas of well-known historians on colonial history in general and on Latin America in particular, devoting their discussion to abstract levels and general schemes of interpretation. These are not tested against case studies.

The second volume, *Economías de exportación y desarrollo capitalista*, divides Latin American economic history into peripheral capitalist development (independence to 1869) and full integration into the world capitalist economy (1870 to the present). Although the focus is again on common lines of interpretation, Cardoso and Pérez Brignoli describe liberal reforms for each of nine countries and colonization in five nations to sustain their hypothesis that the reforms and colonization transformed Latin American economies into systems of dependent capitalism.

The authors affirm that industrial development has not significantly altered Latin America's place in the world economy because "the system does not create its own technology, but imports it; industrialization does not extend its renovating effect to agriculture . . . ; [and because of the] multinational foreign control of industry." The reification utilized, as well as the level and form of analysis in the quotation, are typical of the book.

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E. BRADFORD BURNS. *The Poverty of Progress: Latin America in the Nineteenth Century*. Berkeley and Los Angeles: University of California Press. 1980. Pp. 183. \$12.95.

E. Bradford Burns's frankly exploratory essay attempts to explain the contemporary poverty and powerlessness of the mass of Latin Americans. This condition, he argues, is a product of the nineteenth-century conflict between those elites who relentlessly pursued a "North Atlantic model" (p. 7) of capitalist development and the "folk societies and their cultures" (p. 6) that represented "life style alternatives more advantageous to the masses than the Europeanized modernization imposed on them" (p. 2).

Burns discusses the elite infatuation with the ideas and benefits of progress, focusing on economic growth, that actually brought a "cosmetic modernization" (p. 33) to Latin America by 1914. The dominant "elite preference" (p. 35) was sustained by racist, Europhile images of society and visions for the future that were fashioned by scholars and adopted by official purveyors of progress. A few intellectuals dissented; they emphasized the goodness of native peoples and positive opportunities inher-

ent in their region's unique resources and environments. Rural patriarchs, whose estates were actual communities and who opposed liberal-directed modernization, also constituted a social force compatible with the welfare of the people. Thus a "working relationship between the patriarchs and the folk" (p. 85) helped keep alive alternatives to liberal policies.

The best and most fully developed chapter is "The Folk Speak," which draws upon conventional sources and folklore. It establishes a case for the "popular alternative" (p. 86). It explains the nature and vitality of Latin America's diverse "mediated [folk] cultures" (p. 88) of "Indo-Afro-European" (p. 6) origin; presents the "popular caudillo" (p. 93) as a political leader sometimes capable of protecting the land, economy, and cultures of the folk; and uses evidence of black culture, slave rebellion, banditry, and millenarian movements to bolster the argument for mass opposition to elite, European-style change. Burns suggests that the downfall of popular leaders like Rafael Carrera in Guatemala, Manuel Belzu in Bolivia, and Francisco Solano López in Paraguay was also a defeat of chances for more democratic, equitable, and authentically native societies rather than a victory of civilizing progressives over petty tyrants and backward peoples.

The Progress of Poverty is thought provoking, yet it may strike some readers as if the author grasps at straws in search of viable alternatives to modernization; others may detect both romanticism (in Burns's admiration of the ideology and communitarian values [p. 11] of agrarian peoples) and conservatism (in his view that rural masses were better off during the late colonial era than during the nineteenth century, when they suffered from "diminishing access to land . . . declining real wages and standards of nutrition" [p. 45]). It is, nevertheless, a valuable interpretation worth serious consideration.

Burns does not survey the literature with which his book may compete for academic standing; at its level of analysis, the historiographical context includes the contrasting hypotheses of Andre Gunder Frank and others on capitalist underdevelopment and of Stanley J. Stein and Barbara H. Stein on the colonial heritage and economic dependence of Latin America.

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LAWRENCE A. CARDOSO. *Mexican Emigration to the United States, 1897-1931: Socio-Economic Patterns*. Tucson: University of Arizona Press. 1980. Pp. xv, 192. Cloth \$19.50, paper \$8.95.

Of the many themes typifying United States-Mexican relations throughout the years, none is more fundamental than Mexican emigration. Lawrence A. Cardoso utilizes conventional and unconventional sources to write a scholarly yet personable account of the ebb and flow of Mexican *braceros* during the 1897-1931 period. In so doing he places in bold relief two seemingly eternal truths: the inability or unwillingness of successive Mexican governments to control the northward exodus of their countrymen and the economic determinism of American attitudes toward the *bracero*.

The Mexican Revolution did indeed unleash a tiger, one that ravaged the nation for more than a decade and forced tens of thousands of people to flee the country for their very survival. As Cardoso clearly illustrates, however, the parameters and push-pull factors of this human flood were operative long before the uprising. The post-1910 exodus would have occurred "with or without the Revolution." By the time the massive upheaval had run its course, Mexico's revolutionary politicians were forced to admit publicly what they had long acknowledged in private: the temporary phenomenon of desperation-based emigration had become institutionalized. The best efforts of successive presidents to halt the nationalistically embarrassing migration proved futile. Landless *braceros* paid virtually no attention to intensive government-sponsored propaganda campaigns warning of Armageddon for the Mexican in *gringolandia*. The grapevine confirmed what the stomach dictated: things simply could not be any worse *al otro lado*.

Cardoso provides ample evidence of the link between America's economic climate and the reception accorded Mexican workers. His treatment of President Obregón's efforts to alleviate the plight of stranded compatriots during the 1921 economic downturn is original and well documented by Mexican diplomatic sources. Attitudes and policies of American interest groups and their government toward the *bracero* are succinctly analyzed and linked to social and economic currents of the times. Thanks ultimately to the State Department and a strange amalgam of interests, the Mexican was never included under the immigration quota system of the 1920s. But he was unceremoniously fired and ordered out of the country when the Great Depression struck.

Throughout the entire scenario of government and pressure group interactions in both countries moves the Mexican emigrant. Cardoso, by personalizing his research through the use of *corridos* and other oral history accounts, reminds the reader that those who fled their tumultuous homeland to work the land and toil in the mines of the Southwest were first and foremost human beings. They left Mexico for the same reasons they still leave Mexico: human

reasons. In fact, the more one reads historical accounts of Mexican emigration, the more one is struck by how little things have changed. Even more to the point, scholars could alter a few names and dates, inflate a few figures, rewrite Cardoso's work in the present tense, top the whole thing off with *petróleo*, and they would have a good analysis of the issue of contemporary U.S.-Mexican relations. As this reviewer was once reminded by a resident of northern Mexico, "There are two things you must never forget: rivers flow south but poor Mexicans flow north."

RICHARD B. CRAIG
Kent State University

CLAUDE LEVY. *Emancipation, Sugar, and Federalism: Barbados and the West Indies, 1833-1876*. Gainesville: University Presses of Florida. 1980. Pp. vi, 206. \$13.50.

Among Britain's former sugar colonies, Jamaica, Trinidad, and British Guiana receive principal attention from scholars writing on the post-emancipation period. In many respects, however, Barbados, the most densely populated of the Caribbean colonies, managed the transition to freedom most successfully: its staple agriculture survived without the recruitment of Asian or African indentured labor, civic amenities improved, and the island preserved its ancient legislative government despite the collapse of similar constitutional structures throughout the Caribbean. The exceptional experience of Barbados is ably reported in this prudent, balanced study, the first modern work of scholarship to concentrate on the island in the post-emancipation decades. Although Claude Levy relies heavily on Public Record Office documents, he has read widely and intelligently in the secondary literature. Barbados is not treated in isolation from other Caribbean colonies, a tendency all too common among scholars focusing on particular West Indian communities. The book is introduced by a sweeping essay that reaches back to the founding of the colony (1627) and provides a sketchy, rather episodic introduction to the work. Chapters are organized chronologically, not topically, a structure that creates a choppy effect as the narrative moves quickly from economic to social to educational and religious themes within each separate period. The book concludes with a detailed survey of the federation crisis. This final section will break new ground for many readers.

The book is very effective in its treatment of the apprenticeship and the grave tension surrounding the termination of all forms of coerced labor in 1838. Levy appreciates the limitations of imperial power. When the Colonial Office presumed to dic-

tate arbitrarily the terms of social and economic change, it only provoked unnecessary obstructionism among colonial legislators. As Levy indicates, the planter elite, although conservative and sometimes resentful, were not incapable of moderate, progressive action when approached in a conciliatory manner by colonial governors and the Colonial Office.

If the book has a general shortcoming, it is its tendency to report historical developments and to acknowledge the opinions and debates of scholars without fully analyzing the broader ramifications of those events or the relative merits of the debates. In dealing with widespread hardships endured in Barbados in the decade after the Sugar Duties Act and during the early 1860s, Levy does not take a sufficiently global view. Too little attention is given to manifold external factors that affected the island: in the first instance, the grave international depression and European famines that depressed export markets while driving up the price of imported food, and, in the 1860s, the American Civil War, which interrupted vital trade while the island was experiencing drought.

The book is densely footnoted and well indexed. It is clearly written, flawlessly printed, and constitutes a worthy addition to the literature.

WILLIAM A. GREEN
Holy Cross College

MONICA SCHULER. *"Alas, Alas, Kongo": A Social History of Indentured African Immigration into Jamaica, 1841-1865*. Baltimore: Johns Hopkins University Press. 1980. Pp. x, 186. \$16.50.

This is more than a study of African migration to Jamaica or indeed a social history of the "recaptives" in the colony during the years under review. It seeks to link the development of certain Afro-Jamaican religious patterns to the influx of approximately 9,700 "recaptured" African slaves, of mostly Yoruban and Central African origin, who were transported to Jamaica from Sierra Leone and St. Helena or "liberated" from foreign slave ships in Caribbean waters. While readily conceding the connection between Myalism and the native Baptist groups that had broken away from their earlier American and British roots, Monica Schuler attempts to show that newer African and even some Christian beliefs acquired in missionary settlements in Africa were introduced into Jamaica by the recaptives. There they reinforced existing cultural survivals in at least two parishes, Westmoreland and St. Thomas, and even gave birth to new cults like Kumina and its offshoot, Pocomania. This suggested dichotomy between Creole Afro-Jamaican

culture and that of the more recently arrived Africans is also looked at in a number of other spheres.

Although entirely plausible, this is the weakest aspect of Schuler's work; as she readily concedes, much more comparative work in the Caribbean needs to be done before any definitive conclusions can be reached. At times there is also an over-reliance on a single interpretation of rather complex events, such as the Morant Bay "rebellion" of 1865. After describing the repression that followed that event, Schuler terminates her study rather abruptly without any statement of her principal conclusions.

Alas, Alas, Kongo, nonetheless, does an excellent job of exposing the hypocrisy of this so-called "rescue operation" of enslaved souls, which in so many respects really served as a device to provide the struggling estates with an inexpensive supply of contract labor. Initially the recruitment of liberated Africans was to be voluntary and brief, with eventual repatriation as an added incentive. Hence Jamaican immigration laws were enacted ostensibly to enforce these provisions. Later, however, Colonial Office directives were ignored when much longer terms of indenture were imposed. This was particularly onerous for orphans under fifteen years old, who were obliged to work without remuneration until the age of twenty-one. On other occasions no protective immigration legislation of any kind was enacted.

In their treatment of indentured Africans, many planters resorted to practices previously used under slavery. These included withholding pay and deducting wages for accommodation, clothing, and medical attention, as well as the re-adoption of corporal punishment. Worse still, perhaps, were the repeated instances of those who benefited most from African indenture—namely the Custodes, Assemblers, and other prominent planters—reneging on their commitments to repatriate their workers. Even the attitude of the Crown appeared to harden after it dropped the voluntary nature of indenture and virtually forced the deportation of recaptives from its two African colonies on the grounds that the latter were becoming overcrowded.

A. J. G. KNOX
University of Calgary

NICHOLAS P. CUSHNER. *Lords of the Land: Sugar, Wine, and Jesuit Estates of Coastal Peru, 1600-1767*. Albany: State University of New York Press. 1980. Pp. ix, 225. Cloth \$39.00, paper \$12.95.

The Jesuits played a varied cultural and religious role in colonial America. Historians have examined their function as educators. But how were their teaching activities financed? Magnus Mörner partly

answered that question in his classic work on the Jesuit enterprise in Paraguay. Unfortunately the La Plata region was at the periphery. What of the operation of the Society of Jesus at the center of royal power in South America? Nicholas P. Cushner attempts to answer that question: "Most books on colonial Latin American economics mention the power and wealth of the Jesuits, but few recount the specifics of that power and wealth, and none have attempted a systematic study of the bases of that wealth. I have tried to do this in the Peruvian context" (p. 4).

The primary purpose of Jesuit engagement in agriculture and trade was to finance their perceived educational task. Rather than collect alms, they produced and distributed sugar, wine, textiles, and mules, and they loaned money. Cushner stresses Jesuit investments in sugar cane and vineyards along the Peruvian coast. The first section deals with the acquisition of land, generally accomplished by donation or direct purchase. The author then expertly details the daily operation of various types of estates. Labor conditions are also studied. Slaves constituted the largest labor force of most coastal estates. Cushner notes that the Jesuits, as did other coastal farmers, tended to increase production as prices fell. The author argues that revenues were not as high as critics of the order have claimed. Jesuit mismanagement contributed to low returns; yet unlike individual investors, the Jesuits had extensive resources to tide them over during periods of general economic decline. Further, the Jesuits did not pay the *alcabala* (sales tax) on their products, which provided a competitive advantage. In a final section the author outlines the trade network linking Jesuit enterprises in Paraguay, Buenos Aires, Tucumán, Peru, and Quito.

When the Jesuits were expelled from the Spanish domains in 1767, their papers were expropriated by the state. Cushner used South American and Spanish records as well as Jesuit manuscripts in Rome. There are detailed account books of individual estates, some encompassing long periods. Any researcher using such extensive material is faced with the problem of integrating it into a coherent account. Cushner condensed his analysis to fewer than 200 pages, including maps, figures, and tables. The result is a monograph in which the author raises more questions than he answers. The detailed analysis of often complex economic cycles remains unelaborated. Yet Cushner projects companion volumes: one centering on Quito textile production, another examining the mule and Jesuit tea trade of La Plata. The three volumes should constitute a significant addition to our understanding of the economic history of colonial Latin America.

NOBLE DAVID COOK
University of Bridgeport

PAUL H. LEWIS. *Paraguay under Stroessner*. Chapel Hill: University of North Carolina Press. 1980. Pp. xi, 256. \$22.00.

Paul H. Lewis's political biography of the longtime president of Paraguay, General Alfredo Stroessner, is rich in historical detail and in its treatment of the regime's internal political dynamics. This is the first full-length study of one of the hemisphere's most durable but least-known dictatorships, and it is clearly the best analysis available of twentieth-century Paraguayan politics.

In explaining Stroessner's rise to power and the longevity of his rule, Lewis emphasizes Paraguay's historic authoritarian tradition, its landlocked isolation and defensiveness that perpetuated praetorianism, and its socioeconomic underdevelopment. Stroessner has now ruled longer (since 1954) than anyone previously in Paraguayan history, an accomplishment Lewis notes is not due solely to repression, and could achieve the dubious distinction of staying in power longer than any previous dictator in all of Latin American history. Lewis's book analyzes how and why his dominance of Paraguay's political life has been so enduring.

Stroessner emerges as a disciplined, skillful, hard-working president, not charismatic, but a good administrator and an astute politician. His political bases are chiefly the military and the Colorado party, but over the years he also won grudging admiration from other sectors for the economic development and stability provided by his regime. He undercut the opposition and achieved widespread acquiescence in his continued rule.

Stroessner's regime rested initially on the traditional bases of patronage, favoritism, and coercion. But Lewis shows how, as the country and the regime "developed," it began to use more sophisticated—even totalitarian—techniques: a mass party, the manipulation of public opinion, and technologically advanced methods of control. The picture Lewis draws of this autocratic and sometimes harsh regime is not pretty, but it does enable us to understand it in context. It also makes us wonder why more attention has not been paid to it and its change processes, for after Stroessner Paraguay cannot go back to its earlier, more traditional past.

Although Lewis mentions these themes occasionally, one wishes he had also placed the Stroessner regime in a broader comparative framework, in terms of the new literature on Latin American authoritarianism, corporatism, and bureaucratic statism. The absence of such a comparative perspective is the major weakness of what is otherwise an excellent study: solid, factual, balanced. A volume that systematically compares such authoritarian modernizers as Vargas, Perón, Somoza, Trujillo,

Ubico, Stroessner, Ibañez, and the Mexican PRI would be a welcome addition to the literature.

HOWARD J. WIARDA
University of Massachusetts

DONNA J. GUY. *Argentine Sugar Politics: Tucumán and the Generation of Eighty*. Tempe: Center for Latin American Studies, Arizona State University. 1980. Pp. xii, 162.

The title aptly describes this work since it is a study of government policies, both provincial and national, that affect the development of the sugar industry. No attempt is made to explain the economic significance of sugar for the province of Tucumán. The sugar industry of Tucumán represents an ideal focus for the study of Argentina at the end of the nineteenth century, for it allows the examination of a regional economy within a national context and the study of the types of national and provincial policies that sought to develop both industry and region. The failure of national policies to assure economic and political stability of interior provinces like Tucumán and to integrate the provinces into the nation left Argentina at the beginning of the twentieth century still divided between the interior and littoral provinces.

Although the research is impressive and the topic significant, Donna J. Guy leaves the reader with many questions unanswered. The monograph has no clearly defined thesis or theoretical framework. In spite of a preface that seeks to clarify direction, the work is difficult to follow. It is never clear why a particular organization is used or certain information included or excluded. At times, it appears that a series of barely related issues are simply tied together by a chapter title.

The subtitle, *Tucumán and the Generation of Eighty*, suggests a focus for the monograph. Guy defines the "Generation of Eighty" as the "provincial and Porteño elites who held power in Argentina from 1874 to 1916" and who "created a national capital and devised a workable formula for peace" (p. 3). Yet the elite remains elusive. One never obtains a clear picture of who the individuals were, what they wanted, or how much they had in common. Are the men pictured in the book all or a few of the "Generation of Eighty"?

Although Guy refers to the sugar industry as fitting imprecisely within the general pattern of Latin American sugar industries, she never relates or compares. Nor is one quite certain as to how Tucumán, sugar, and sugar policies fit with the rest of the policies of the "Generation of Eighty."

Argentine Sugar Politics: Tucumán and the Generation of Eighty is a significant topic. The author's archival research provides information on interior develop-

ment, significant political events of the era, and some of the individuals involved in sugar politics. One wishes, however, that Guy had been more daring in her interpretation and analysis and broader in the questions asked and answered.

VERA BLINN REBER
Shippensburg State College

ROBERT WEISBROT. *The Jews of Argentina: From the Inquisition to Perón*. Philadelphia: Jewish Publication Society of America. 1979. Pp. xiii, 348.

Robert Weisbrot, a graduate student in American history at Harvard, has written a useful volume on Argentine Jewry based on interviews and observations carried out in 1972. His work was supplemented by further research into the abundant collection of secondary materials on this topic.

Despite a recent heightening of interest in the Jewish history of Latin America, this subfield still suffers from a paucity of objective archival research. In the case of Argentina, home of the largest Jewish community, the nineteenth-century origins and the agricultural colonies of that community have been studiously researched. Jewish Argentines have written prolifically, but their output is of uneven quality. Lost or destroyed documents and private archives that remain closed have precluded a definitive work, particularly for the contemporary period. Weisbrot has examined sufficient materials to produce an acceptable monograph, but the result is disappointing.

The Jews of Argentina eschews historical narrative for the comprehensive, topical approach. Sections on settlers, culture, and country reveal a haphazard organization. The German subgroup, for example, appears as "settlers," while the Sephardic element, which also dates back to the nineteenth century, is treated under "culture." The story of Julio Popper, a mining engineer in Tierra del Fuego, is similarly misplaced under "Zionism" (pp. 89-91). Perón's name, both in the subtitle and as a chapter heading, offers a chronological trap; the epilogue, which should have carried events forward from Perón's death in 1974, is taken from the author's article in *Present Tense* of that same year (pp. 9-11).

Several methodological difficulties also surface. Failure to specify the archival holdings of institutions weakens the bibliography. An uncritical acceptance of the sources results in several factual errors (for example, population size) as well as trite statements and excessive trivia. Often Weisbrot allows "impressionistic evidence" to supplant written evidence (namely, p. 157). Additionally he tends to overlook the South American continent while favoring inappropriate comparisons with Jewish experience in the United States. Thus his discussion of

low enrollment in the Jewish schools (p. 278) fails to take into account the relative parochialism of the Latin American countries.

Weisbrot has indeed absorbed much of the varied mosaic of Jewish life in Argentina. Although he frequently views developments through the harsh lens of recent civil unrest, the final section—which could stand alone as an independent treatise—is a superb historic narrative of the dynamic interplay between the Jewish community and the wider society. This book, however, seems to have been hurriedly brought to press, perhaps to coincide with dramatic political events. As such, it does not meet its stated pretense of being a major reference work on the history of Jews in Argentina.

BERNARD D. ANSEL

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Buffalo*

ROBERT A. POTASH. *The Army and Politics in Argentina, 1945–1962: Perón to Frondizi*. Stanford: Stanford University Press. 1980. Pp. ix, 418. \$25.00.

The long-awaited second volume in Robert A. Potash's major analysis of the Argentine military's political role since 1928 will disappoint no one concerned with unraveling the often perplexing turns and twists of that country's recent development. As in the first volume, Potash has eschewed the use of models or other social science methodology in favor of historical narrative and analysis, now heavily documented by oral sources. A basic message emerges far more clearly than in his study of the pre-Perón years—namely, constant factionalism and steadily increasing division within the military establishment. This theme, entwined with continued emphasis on the loss of faith in the democratic process, does nothing to dispel the sense of fatalism and futility that the narrative of military involvement in politics casts over the reader.

Readers will be well advised to familiarize themselves with Potash's first volume, since he launches into the complex events that followed the resuscitation of Perón's political career on October 17, 1945, with only the barest of introductions. In dealing with the first major event of the 1945–62 period, Potash makes clear that, although guaranteeing free voting at the polls, the army also created many of the conditions favoring Perón's 1946 election. From the vantage point of the military, however, external events doubtless played a decisive role: the assessment that the Blue Book significantly contributed to Perón's narrow victory receives ample reinforcement from Potash's sources.

If Potash's treatment of the ensuing Perón era holds few surprises, it nonetheless proves valuable as a reminder that even Perón faced pressures and re-

ceived divided counsels from his advisers, congress, and the labor movement. During his initial years, however, he did not apparently have to concern himself with a divided military. But, beginning in 1949, fissures started to develop, including reservations about the 1949 constitution and resistance to measures taken to integrate the military within the Perónist movement. With superb mastery of the available evidence, Potash outlines the process of polarization within the military during 1951 that climaxed with the poorly planned and executed uprising of September 28. The ensuing narrative of Perón's second presidency contains numerous bits of fascinating detail: the Suárez conspiracy of February 1952 and Perón's response in a remarkable and irresponsible contingency plan; psychological reasons for Perón's anticlerical turn and his abandonment of the 1954 period of political reconciliation; the complex constellation of interests and officers that finally coalesced to bring Perón down; and the critical role played by the military junta in peacefully transferring power to General Eduardo Lonardi.

Despite substantial contributions made by this volume to delineating Perón's relationship with the armed forces, especially during the final months of his regime, the most interesting and valuable pages deal with the post-1955 period. Potash presents extensive new evidence on the navy's desperate efforts to retain a share in the decision-making processes, details General Pedro Aramburu's masterful handling of intra-army clashes and his gradual assertion of control over the military, and explains the complex maneuverings behind Arturo Frondizi's eventually successful candidacy for the presidency in 1958. Particularly fascinating are Potash's explanations of Frondizi's subsequent compromises with and surrenders to military pressures. As one follows Potash through the maze of political intrigues, intraservice and personal rivalries, and divided loyalties, one relives the Frondizi miracle of plucking victory at the eleventh hour from the shambles of disaster—from his clever unseating of Admiral Arturo Rial in July 1958 to his incredible removal of General Carlos Toranzo Montero in March 1961.

Despite a still sharply divided military, Frondizi apparently felt, by mid-1961, sufficiently in control to pursue an assertive foreign policy, often in direct contradiction to military interests. Here, hypothesizes Potash, lay the seeds of his undoing. His secret interview with Ernesto "Ché" Guevara in August 1961 and Argentina's refusal to vote for immediate exclusion of Cuba from the OAS in January 1962 precipitated new crises within the military and led to a hardening of anti-Perónist and anti-Communist lines. When Frondizi's party failed in its March 1962 gamble to defeat Perónist candidates in the

critical gubernatorial and congressional elections, the president had to violate his own commitment to legality in order to decree interventions and nullify those elections. Now even the most legally minded officers demanded his resignation. But even in that final crisis, which led to his removal from the presidency at the end of March, Potash makes it clear that Frondizi, by yielding and compromising, once again came very close to surviving.

In his research and analysis, Potash has pieced together a highly complex puzzle. Doubtless some

will disagree with his hypotheses and reasoning; a few may find occasional shortcomings in facts or interpretations; others will regret the one-dimensional perspective on political conflict when taken primarily from the vantage point of the armed forces. But all can come a little closer to understanding present-day Argentina by reading this book and appreciating the wealth of information that it contains.

JAMES R. SCOBIE

*University of California,
San Diego*

Collected Essays

These volumes, recently received in the *AHR* office, do not lend themselves readily to unified reviews; the contents are therefore listed. Other similar volumes that are amenable to reviewing will be found in the review section.

G. S. ROUSSEAU and ROY PORTER, editors. *The Ferment of Knowledge: Studies in the Historiography of Eighteenth-Century Science*. New York: Cambridge University Press. 1980. Pp. xiii, 500. \$39.50.

ROM HARRÉ, Knowledge. SIMON SCHAFFER, Natural Philosophy. STEVEN SHAPIN, Social Uses of Science. G. S. ROUSSEAU, Psychology. W. F. BYNUM, Health, Disease and Medical Care. JACQUES ROGER, The Living World. ROY PORTER, The Terraqueous Globe. H. J. M. BOS, Mathematics and Rational Mechanics. J. L. HEILBRON, Experimental Natural Philosophy. MAURICE CROSLAND, Chemistry and the Chemical Revolution. ERIC G. FORBES, Mathematical Cosmography. D. S. L. CARDWELL, Science, Technology and Industry.

MEL ALBIN, editor. *New Directions in Psychohistory: The Adelphi Papers in Honor of Erik H. Erikson*. Lexington, Mass.: D. C. Heath. 1980. Pp. xx, 217. \$16.00.

FRED WEINSTEIN, On the Social Function of Intellectuals: A Consideration of Erik H. Erikson's Contribution to Psychoanalysis and Psychohistory. JOEL KOVEL, Marx and Freud. TALCOIT PARSONS, The Articulation of the Personality and the Social-Action System: Sigmund Freud and Max Weber. ERNEST S. WOLF, Psychoanalytic Selfobject Psychology and Psychohistory. MILES F. SHORE, Emerging Paradigms in Psychoanalysis and Related Fields. STANLEY A. RENSHON, Life History and Character Development: Some Reflections on Political Leadership. GERALD M. PLATT, Thoughts on a Theory of Collective Action: Language, Affect, and Ideology in Revolution. CUSHING STROUT, Psyche, Clio, and the Artist. DAVID F. MUSTO, Continuity Across Generations: The Adams Family Myth. HOWARD FEINSTEIN, Words and Work: A Dialectical Analysis of Value Transmission between Three Generations of the Family of William James. SUSANNE HOEBER RUDOLPH and LLOYD I. RUDOLPH, Authority and the Transmission of

Values in the Rajput Joint Family. DOMINICK CAVALLO, Sexual Politics and Social Reform: Jane Addams, from Childhood to Hull House. EDWARD SHORTER, Women's Diseases before 1900.

ARTHUR E. IMHOF, editor. *Mensch und Gesundheit in der Geschichte*. (Abhandlungen zur Geschichte der Medizin und der Naturwissenschaften, number 39.) Husum: Matthiesen Verlag. 1980. Pp. viii, 415. DM 69.

ARTHUR E. IMHOF, Einleitung. JEAN-NOËL BIRABEN, Histoire des classifications de causes de décès et de maladies aux XVIII^e et XIX^e siècles. VINCENT-PIERRE COMITI, Importance de connaissances médico-biologiques actuelles pour une étude critique de pathocénose historique. ØIVIND LARSEN, Die Krankheitsauffassung und ihre historische Interpretation. Ein Auswertungsmodell Aufgrund von norwegischen Medizinalberichten aus dem 19. Jahrhundert. JEAN-PIERRE GOUBERT, L'hygiène de l'eau dans la France contemporaine d'après les thèses de doctorat de la Faculté de Médecine de Paris (1830-1940). ROBERT W. LEE, Medicalisation and Mortality Trends in South Germany in the early 19th Century. HEINRICH POMPEY, Pastoralmedizin—der Beitrag der Seelsorge zur psycho-physischen Gesundheit. Eine bibliographisch-historische Analyse. OIVIE TURPEINEN, Die Sterblichkeit an Pocken, Masern und Keuchhusten in Finnland in den Jahren 1751 bis 1865. BENGT I. LINDSKOG, Mortalitätsanalyse einer südschwedischen Bevölkerung 1749-1818. ALFRED PERRENOUD, Contribution à l'histoire cyclique des maladies. Deux siècles de variole à Genève (1580-1810). CLAUDE BRUNEEL, Un problème de gouvernement: Le pouvoir face à l'épidémie de fièvre putride à Bruxelles en 1772-1773. ALAIN BIDEAU, Variables familiales de la mortalité infantile. REINHARD SPREE, Die Entwicklung der differentiellen Säuglingssterblichkeit in Deutschland seit der Mitte des 19. Jahrhunderts (Ein Versuch zur Mentalitäts-Geschichte). JACQUES GÉLIS, Regard sur l'Europe médicale des Lumières: la collaboration internationale des accoucheurs et la formation des sages-femmes au XVIII^e siècle. MARIE-FRANCE MOREL, Mère, enfant, médecin: la médicalisation de la petite enfance en France (XVIII^e-XIX^e siècles). Françoise Loux, Recours convergents à la médecine officielle et à la médecine parallèle en matière de soins aux enfants en France (XIX^e-XX^e siècles). INGER WIKSTRÖM-HAUGEN, Patienten im Sahlgrenschen Krankenhaus Göteborg 1782-1822. TOBY GEL-

FAND, Les caractères originaux d'un hospice parisien à la fin de l'Ancien Régime.

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CELIA DAVIES, Introduction: The Contemporary Challenge in Nursing History. CHRISTOPHER MAGGS, Nurse Recruitment to Four Provincial Hospitals 1881-1921. KATHERINE WILLIAMS, From Sarah Gamp to Florence Nightingale: A Critical Study of Hospital Nursing Systems from 1840 to 1897. MITCHELL DEAN and GAIL BOLTON, The Administration of Poverty and the Development of Nursing Practice in Nineteenth-Century England. CELIA DAVIES, A Constant Casualty: Nurse Education in Britain and the USA to 1939. MICK CARPENTER, Asylum Nursing before 1914: A Chapter in the History of Labour. PAUL BELLABY and PATRICK ORIBABOR, "The History of the Present"—Contradiction and Struggle in Nursing. MARGARET CONNOR VERSLUYSEN, Old Wives' Tales? Women Healers in English History. JANET FOSTER and JULIA SHEPPARD, Archives and the History of Nursing. CHARLOTTE KRATZ, Epilogue.

GORDON E. CHERRY, editor. *Shaping an Urban World*. New York: St. Martin's Press. 1980. Pp. xii, 259. \$27.50.

GORDON E. CHERRY, Introduction: Aspects of Twentieth-Century Planning. PETER MARCUSE, Housing Policy and City Planning: The Puzzling Split in the United States, 1893-1931. BLAINE A. BROWNELL, Urban Planning, the Planning Profession, and the Motor Vehicle in Early Twentieth-Century America. DIETER REBENTISCH, Regional Planning and its Institutional Framework: An Illustration from the Rhine-Main Area, 1890-1945. W. HOUGHTON-EVANS, Schemata in British New Town Planning. SHUN-ICHI J. WATANABE, Garden City Japanese Style: The Case of Den-en Toshi Company Ltd., 1918-28. GERD ALBERS, Town Planning in Germany: Change and Continuity under Conditions of Political Turbulence. GORDON E. CHERRY, The Place of Neville Chamberlain in British Town Planning. SUSAN M. CUNNINGHAM, Brazilian Cities Old and New: Growth and Planning Experiences. A. D. KING, Exporting Planning: The Colonial and Neo-Colonial Experience. JOHN COLLINS, Lusaka: Urban Planning in a British Colony, 1931-64. ROBERT FISHMAN, The Anti-Planners: The Contemporary Revolt against Planning and its Significance for Planning History.

ANTHONY SUTCLIFFE, editor. *The Rise of Modern Urban Planning, 1800-1914*. New York: St. Martin's Press. 1980. Pp. xi, 235. \$27.50.

ANTHONY SUTCLIFFE, Introduction: The Debate on Nineteenth-Century Planning. DAVID R. GOLDFIELD, Planning for Urban Growth in the Old South. PETER BREITLING, The Role of the Competition in the Genesis of Urban Planning: Germany and Austria in the Nineteenth Cen-

tury. DONATELLA CALABI, The Genesis and Special Characteristics of Town-Planning Instruments in Italy, 1880-1914. JOHN NELSON TARN, Housing Reform and the Emergence of Town Planning in Britain before 1914. P. J. SMITH, Planning as Environmental Improvement: Slum Clearance in Victorian Edinburgh. FRANZISKA BOLLEREY and KRISTIANA HARTMANN, A Patriarchal Utopia: The Garden City and Housing Reform in Germany at the Turn of the Century. WILLIAM H. WILSON, The Ideology, Aesthetics and Politics of the City Beautiful Movement. HELEN MELLER, Cities and Evolution: Patrick Geddes as an International Prophet of Town Planning before 1914.

STUART ALBERT and EDWARD C. LUCK, editors. *On the Endings of Wars*. (National University Publications, Series in Political Science.) Port Washington, N.Y.: Kennikat Press. 1980. Pp. 174. \$17.50.

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The following collections of documents, bibliographies, and other similar works were received by the *AHR* between November 8, 1980, and January 21, 1981. Books that will be reviewed are not usually listed, but listing does not necessarily preclude subsequent review.

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TO THE EDITOR:

I never used the awkward combination of words "the old landed and social classes" that C. Ernest Dawn put in quotation marks in his review of my book on Iraq (*AHR*, 85 [1980]: 439-40). I also never referred to the sheikhs or the Ottoman officials as the "old classes." I used "old classes" to refer to the pre-1958 landowners or men of money and commerce and the term "status-group" to refer to such components of the landed class as the sheikhs and the "aristocrat" officials or such components of the mercantile class as the *chalabis* and *nouveaux riches*.

Moreover, tables 5-1 and 5-3 on landholdings in 1958 do *not* bear incorrect titles. In the heading of table 5-1, I used the terms "privately held land" instead of "privately owned land" precisely to cover private lands and land of unsettled title or state lands *in private hands*. Table 5-3 comprises only private estates.

I should be obliged if Professor Dawn would indicate the pages on which "exploitation and repression of the masses are depicted as the main causes of the monarchy's failure." I do not understand why Dawn should have attributed to me this imaginary "main thesis" and ignored completely the many and different reasons that I actually gave for the monarchy's failure: "its moral divorce from the mass of the politically conscious strata of the people . . . ; [the weakening] of the loyalty of the very elements—the officials, the army, and even the police—through whom it exercised its will upon the country" (p. 34); its linking of its fortunes with the English in the Anglo-Iraqi war of 1941 thus vitiating itself, in the eyes of the younger pan-Arab officers, as "a symbol of the nation" (p. 766); "its alliance with the sheikhs . . . [which undermined its ability] to play a unifying social role . . . and condemned the majority of the inhabitants of the coun-

try (the peasants) to depressed conditions" (p. 32); "its too close . . . identification with a waning British imperial order . . . and the inherent inability of its ruling element to cope effectively with serious social unbalances . . ." (p. 351).

At the same time, I think that, on a closer reading of Iraq's history, Professor Dawn would be less prone to dispute—as is implied by one of his generalizations—that the change of government in 1948 was induced by tempestuous mass protests, that the violent risings of 1948 and 1952 were intimately related to the suffering of the urban poor (see pp. 470-75), or that the populace played an effective role in the overthrow of the monarchy on the day of July 14, 1958 (pp. 804-05). It is interesting that Dawn should have referred to the events of 1948 as "riots," which is clearly an inappropriate term with strong elitist and antidemocratic overtones and, in great measure, undescriptive of what actually happened.

In my opinion, I did not "overstress" the Communists but merely, as I duly warned my readers (p. xxi), "represent their history on a large scale." This is a permissible procedure inasmuch as in the relevant sections of the book the Communists form explicitly my chief topic of discourse. Moreover, the available evidence indicates unequivocally that from 1944 to 1948 and from 1956 to 1963 the Communists constituted, in terms of number of supporters and sympathizers, the most important political party in Iraq, notwithstanding the repeated severe blows inflicted upon them by the political police. Professor Dawn noted correctly that their leadership was rent by rivalries but did not note that the splits coincided with periods of political quiescence and that in moments of acute crisis, as in 1948 or 1958-59, their party cohered. That the Communists did not translate their mass support into enduring political gains was due to their relatively thin representation or unstable foothold in the officer corps and to the subordination of their behavior to the international requirements of Soviet policy. Anyhow, their importance cannot be measured in terms of the pulling or nonpulling of coups but in the light of such factors as their impact on the organizational structure of the Ba'th party and

the categories of thought not only of the Ba'thists or the Kurdish Democrats but also of such disparate figures as the secularly minded 'Abd-ul-Karīm Qāsin and the out-and-out Muslim 'Abd-us-Salām 'Aref. The Communists represent in Iraq a deep-reaching tradition that even Ṣaddām Ḥusayn and his regime have found difficult to uproot.

HANNA BATATU
American University of Beirut

PROFESSOR DAWN REPLIES:

Hanna Batatu's first objection to my review appears to be that it distorts his analysis of the class dimension of Iraqi politics before 1958. He did not use the phrase, "the old landed and social classes," and he did include both "social classes" and "status groups" in his analysis. A review cannot summarize all of the refinements of a long analysis but must point out the main features. This, I think, my review does. The review highlights Batatu's most important categories—the sheiks, the Ottoman official families and so on—and ascribes to them the roles that he ascribed to them.

The titles of tables 5-1 and 5-3 are not incorrect, but they are misleading. Professor Batatu, in his major description of landholding on pages 53-57 used "privately held land" twice (pp. 55, 57), without definition, and he did not define the term in his tables. Nowhere did he say that the phrase refers to all agricultural holdings, which in fact it does. That the term is not used for all landholdings is suggested by the title of table 5-2, "Distribution of Landholdings in Kūt Province in 1958." Why refer to "privately held agricultural land" in one place and "landholdings" in another if they are in fact the same? Ambiguity could have been avoided by following common usage, either "agricultural holdings" or "landholdings." State lands and lands of unsettled title constituted a significant portion of all holdings and have been very important in Iraqi politics before and after 1958. Accordingly, it was and is useful to point out that these lands are included in table 5-1. This fact is not easily apparent from Batatu's exposition, and a large proportion of the many who use his valuable data will not have access to the published Iraqi agricultural census, which is not readily available.

Professor Batatu in many places depicted the monarchy as exploiting the masses and emphasized the discontent of the masses as reason for the monarchy's failure: "The history of Iraq became henceforth [after the 1930s] largely the history of Baghdad, and its arresting feature the transient but recurring sovereignty of the masses of the capital city" (p. 119). Note the passage on page 351, a part of which Batatu cites in his letter: "... it [the monarchy] suffered from the inherent inability of its ruling element to cope effectively with serious social

unbalances, to which pointed the recurrent urban uprisings and the intermittent agrarian unrest. . . ." Further, "In brief the principal ex-Sharīfians had become very much a part of the agricultural and moneyed vested interests, and less and less conscious and comprehending of the little people and of the problems and difficulties of their daily life" (p. 353). For additional examples, see pages 470, 472, 473, 475, 476, and 806. Batatu did point out other sources of opposition to the monarchy, but the emphasis is heavily on mass discontent. Clearly, it is fair to say, as I did, "exploitation and repression of the masses are depicted as the main causes of the monarchy's failure."

It is not clear whether Professor Batatu, in the fourth paragraph of his letter, is maintaining that exploitation-caused mass discontent was the main cause or a main case of the monarchy's failure. In either case, I believe he fails to delineate the relation of violent outbreaks to Iraqi politics. As indicated in my review, popular demonstrations were organized and assisted by members of upper-status groups, both opposition and ruling circle, and rivalries within the government were of major significance, especially during the 1948 disturbances. ("Portsmouth Treaty riots" is much more likely to be known to the hoped-for general reader than is the Arabic *al-Wathbah*.) Governments have been changed solely by army coup. In 1958, the masses did not take to the streets until the coup had succeeded.

I see no need to change my assessment of the Communists' role. To add a bit to the review, Professor Batatu related several instances of establishment politicians providing protection to Communist leaders under the monarchy. Since 1958, those in power in Iraq have alternately utilized and repressed the Communists, thus far, at least, to the sole advantage of those in power.

C. ERNEST DAWN
*University of Illinois,
Urbana-Champaign*

ERRATUM:

In Doris Goldstein's review of Deborah Wormell's *Sir John Seeley and the Uses of History* (AHR, 86 [1981]: 135-36), the publication date for *The Expansion of England* should have been given as 1883 not 1833.

THE EDITORS

EDITORS' NOTE:

The two sequences of articles devoted to comparative history in theory and practice (AHR, 85 [1980]: 753-857, 1055-1166) have generated an unusually large number of letters to the editor. The editors have decided, for coherence, to publish all of the letters and replies together next fall.

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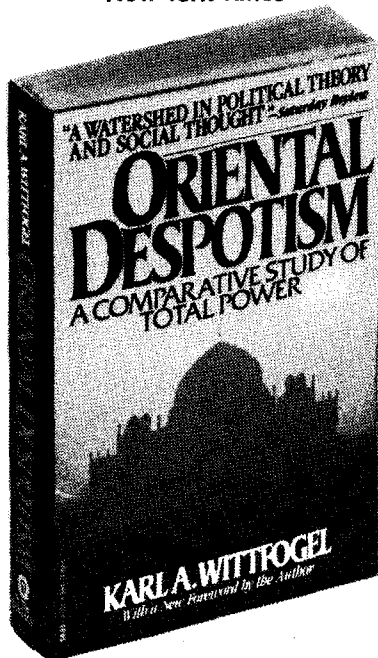
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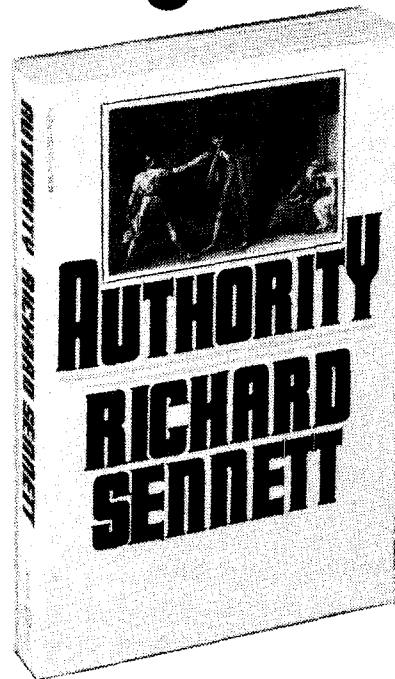
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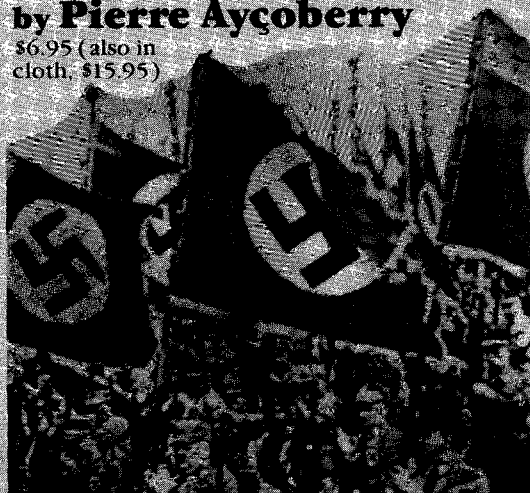
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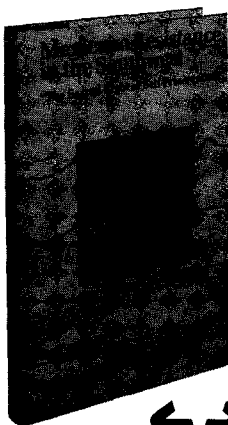
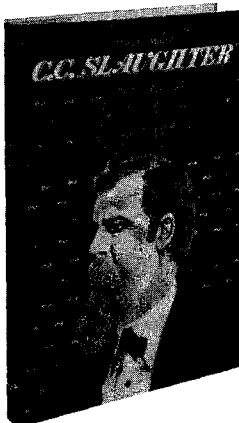
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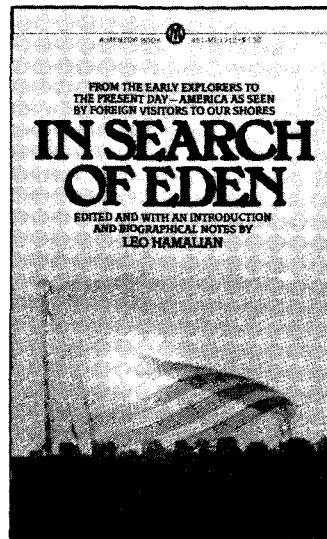
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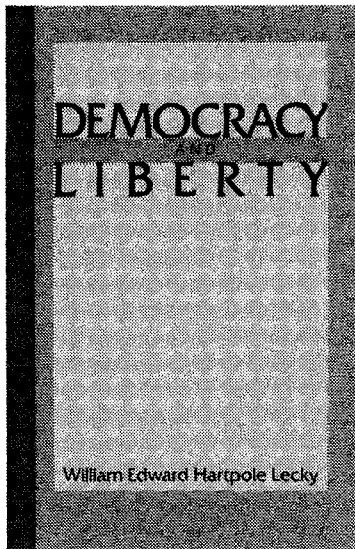
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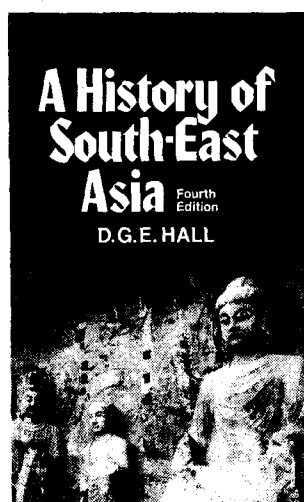
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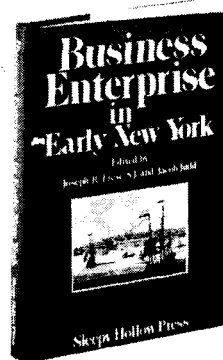
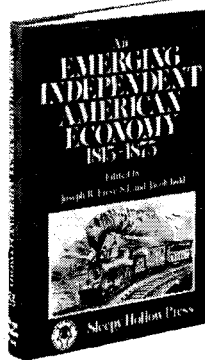
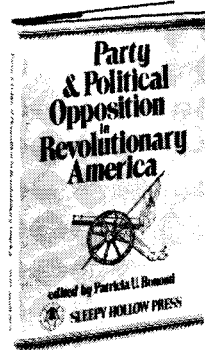
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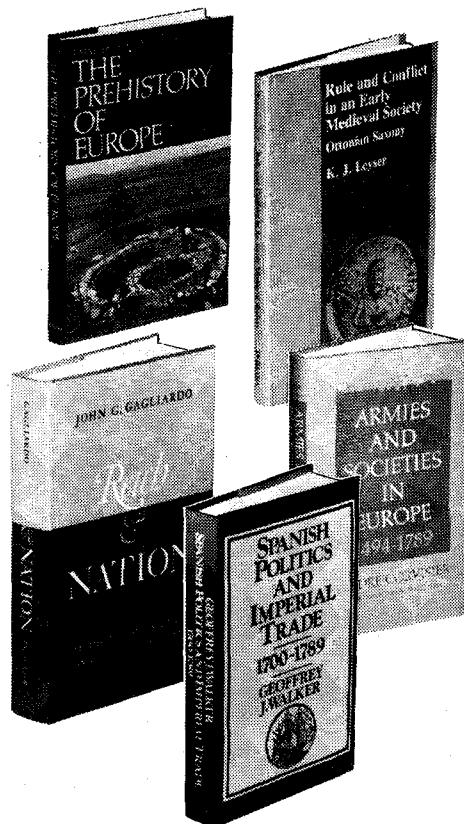
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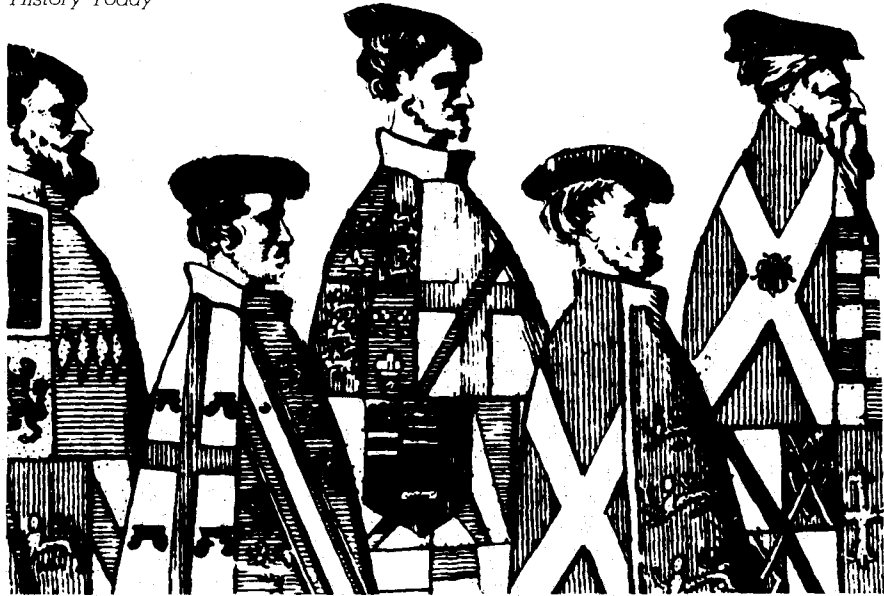
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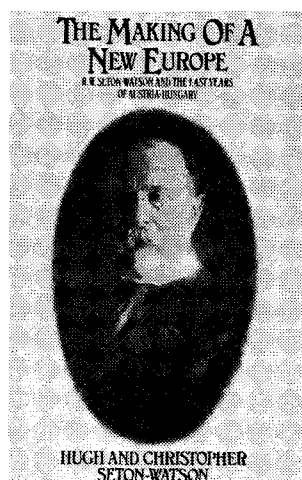
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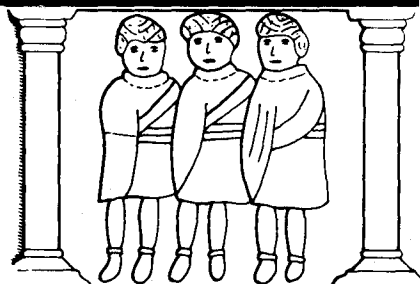
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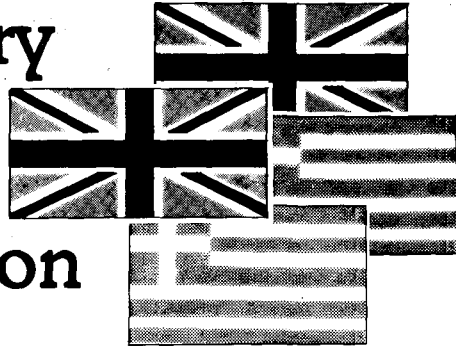
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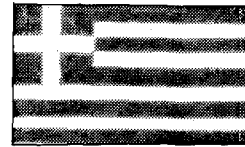
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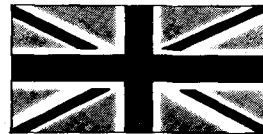
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